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J. R. HIND, Esq., F.R.S., President, in the Chair.

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Staff-Commander W. B. Goldsmith, R.N., F.R.G.S, Royal Yacht "Elfin," Portsmouth;
Elijah Howarth, Esq., Public Museum, Sheffield;
Sydney T. Klein, Esq., 24 Belsize Park, N.W., and The Rock, Reigate Hill, Surrey;
Dr. Nicolas de Konkoly, Buda Pest, Hungary; and
The Rev. Joseph Reed, M.A., H.M.S. "Triumph;"

were balloted for and duly elected Fellows of the Society.

On the Bearing of Ancient Eclipses, and especially that of Agathocles, on the Moon's Motion. By Signor J. Celoria.

(*Extract from a letter addressed to the President.*)

J'ai l'honneur de vous envoyer un Mémoire sur quelques éclipses solaires anciennes, et sur celle d'Agathocle en particulier. Dans le même je commence pour référer des documents anciens, tirés des ouvrages de Cléomède, de Théon et de Ptolomée. Ce sont des documents qui individuent avec la plus grande évidence une

éclipse du Soleil qui a été : 1° totale sur l'Hellespont ; 2° visible et observée à Alexandrie ; 3° utilisée par Hipparque dans ses calculs ; la date seule y reste obscure et ignorée.

L'incertitude de notre théorie lunaire, l'indétermination des documents qui regardent toutes les éclipses de Soleil anciennes, l'importance qu'aurait sans doute dans la théorie de la Lune une éclipse solaire ancienne bien individuée, m'ont persuadé à faire des recherches sur la date de l'éclipse à laquelle se rapportent les documents indiqués.

J'ai par conséquence calculé toutes les éclipses totales de Soleil dont les dates arrivent entre le 1 janvier de l'année 331 et le 31 décembre de l'année 101 A. C. ; ou, selon la méthode astronomique de numéroter les années, entre le commencement de l'année −330, et la fin de l'année −100.

Parmi les éclipses calculées il n'y en a pas une seule dont, après les Tables de la lune de M. Hansen, la zone de totalité soit passée sur l'Hellespont ; on peut par conséquence conclure qu'aussi dans les siècles qui précèdent notre ère les Tables lunaires de M. Hansen ne représentent pas suffisamment bien les faits observés, et qu'aussi pour les époques anciennes il faut aux longitudes tabulaires de M. Hansen apporter une correction sensible. C'est une conséquence qu'on pouvait déjà présupposer, et que maintenant on peut dire démontrée rigoureusement ; à la conséquence même sont aussi favorables les calculs exécutés sur les éclipses du 14 mars de l'année −189, et du 29 août de l'année −115.

Des calculs exécutés et de la discussion faite dans le chapitre iv du Mémoire découle une seconde conséquence remarquable, c'est à dire que parmi les éclipses anciennes l'éclipse d'Agathocle est celle qui peut mieux satisfaire aux faits démontrés et affirmés par les documents indiqués, et que dans l'éclipse d'Agathocle la zone de totalité passa sur l'Hellespont.

Cela démontré, je développe une relation différentielle entre le déplacement $d\mathrm{D}$ de la zone de totalité pris sur la perpendiculaire à la direction de la ligne centrale, la correction dv de la longitude moyenne de la Lune, et la correction $d\Omega$ de la longitude du nœud. Le signe du déplacement $d\mathrm{D}$ est positif lorsque la zone de totalité doit être transportée vers le nord.

J'applique cette formule aux éclipses du 3 juin 1239, et du 6 octobre 1241, jadis étudiées par moi, et je trouve les deux relations—

$$+35'{\cdot}4 = -\ 1{\cdot}6786\,dv - 5{\cdot}3743\,d\Omega,$$
$$-42'{\cdot}6 = +30{\cdot}5820\,dv - 7{\cdot}1803\,d\Omega,$$

dans lesquelles les quantités $+35'{\cdot}4$ et $-42'{\cdot}6$ ne sont pas absolues, mais susceptibles d'augmentation ou de diminution, puisqu'elles dépendent de l'interprétation d'anciens documents ; leurs variations pourtant ne pourraient être trop grandes, et les équations écrites ne peuvent par conséquence être satisfaites que par des valeurs négatives de dv, ce qui s'accorde avec les recherches

de M. Newcomb, desquelles résulte aussi que les longitudes moyennes tabulaires de M. Hansen demandent pour des époques analogues à celles des éclipses de 1239 et de 1241 des corrections négatives.

L'éclipse d'Agathocle donne entre les quantités considérées la relation suivante :

$$d\mathrm{D} = -5{\cdot}42633\,dv - 5{\cdot}42542\,d\mathfrak{Q}.$$

Dans cette relation la quantité dD doit être positive ; on voit par conséquence que pour la satisfaire, lorsqu'on ne veut admettre pour $d\mathfrak{Q}$ des valeurs trop grandes et incompatibles avec l'état actuel de notre théorie lunaire, il faut nécessairement prendre pour dv une valeur négative. Cette conséquence maintenant démontrée me paraît d'une importance capitale : non seulement dans les siècles postérieurs à notre ère, mais aussi dans les siècles antérieurs à l'ère même, les longitudes moyennes tabulaires de M. Hansen demandent une correction négative.

De la simple relation entre dD, dv et $d\mathfrak{Q}$, démontrée pour l'éclipse d'Agathocle, on ne peut naturellement rien déduire de certain sur les valeurs numériques de dv et $d\mathfrak{Q}$, mais il sera pourtant possible de voir si les hypothèses qu'on peut maintenant faire sur les valeurs mêmes sont plus ou moins conciliables avec la relation démontrée.

Je prends pour le coefficient de l'accélération séculaire du mouvement moyen de la lune la valeur théorique $6''{\cdot}176$ au lieu de la valeur adoptée par M. Hansen, et je combine la valeur de dv qui correspond à cette hypothèse avec des valeurs différentes de dD. Si on la combine avec la valeur de dD égale à $+188'$, c'est à dire si on transporte la ligne centrale de l'éclipse sur la pointe méridionale de l'Hellespont, on trouve pour $d\mathfrak{Q}$ la valeur $+150''{\cdot}27$; si on la combine avec la valeur de dD égale à $+240'{\cdot}7$, ou si sur la pointe méridionale de l'Hellespont on transporte la limite sud de la zone de totalité, on trouve pour $d\mathfrak{Q}$ la valeur $-432''{\cdot}58$. Selon le premier résultat, le mouvement du nœud adopté par M. Hansen exigerait, dans chaque siècle postérieur à 1800 et à partir de 1800 même, une correction négative et égale en valeur absolue à $7''{\cdot}12$; selon le second résultat, le même mouvement du nœud exigerait au contraire une correction séculaire positive et égale à $20''{\cdot}50$. Mais, en adoptant pour dD la valeur $+188'$ et *à fortiori* l'autre $+240'$, on transporte la zone de totalité de l'éclipse tellement au nord que le passage d'Agathocle devient impossible au sud de la Sicile. Or, quoiqu'il y ait des arguments favorables au passage d'Agathocle au nord de la Sicile, il serait pourtant arbitraire d'affirmer avec certitude, après la seule narration de Diodore, qu'Agathocle a suivi l'une plutôt que l'autre route. Pour se tenir à la lettre des documents existants, et pour ne pas faire pour le moment des hypothèses arbitraires, il faut déplacer la zone de totalité de manière qu'elle recouvre l'Hellespont, et qu'elle passe dans le même temps sur les deux positions

probables d'Agathocle, quelle que soit la route qu'il a suivie. A cet effet il suffit de prendre pour dD la valeur $+153'$ (valeur qui, combinée avec la valeur de dv qui correspond à la valeur théorique du coefficient de l'accélération séculaire, donne pour $d\mathfrak{S}$ la valeur $+537''{\cdot}28$) et pour la correction séculaire du mouvement du nœud adopté par M. Hansen une valeur négative et égale à $25''{\cdot}46$.

L'incertitude de cette dernière valeur n'est pas grande lorsqu'on ne veut pas abandonner l'hypothèse du passage d'Agathocle au sud de la Sicile, et lorsqu'on n'oublie pas la grandeur de l'éclipse observée à Alexandrie, grandeur qui s'oppose à un trop grand déplacement de la zone de totalité vers le nord. Si on fait

$$d\mathrm{D} = +153' + x,$$

la valeur de la correction séculaire du mouvement du nœud est représentée par l'expression

$$-25''{\cdot}46 + 0{\cdot}0087x,$$

dans laquelle x est une quantité qui ne peut pas même devenir égale à $+300''$ sans rendre impossible le passage d'Agathocle au sud de la Sicile.

Tels sont les résultats principaux contenus dans le Mémoire que j'ai l'honneur de vous adresser, c'est à dire :

(1) La zone de totalité dans l'éclipse d'Agathocle doit passer sur l'Hellespont ;

(2) Non-seulement dans les siècles postérieurs à notre ère, mais aussi dans ceux antérieurs à la même, les longitudes moyennes tabulaires de la lune demandent une sensible correction négative ;

(3) La valeur théorique $6''{\cdot}176$ du coefficient de l'accélération séculaire du mouvement moyen de la lune est conciliable avec l'éclipse d'Agathocle lorsqu'on admet, pour le mouvement du nœud lunaire adopté par M. Hansen, une correction égale à $-25''$ dans chaque siècle postérieur à 1800 et à partir de 1800 même ;

(4) La valeur théorique du coefficient de l'accélération séculaire du mouvement moyen de la lune est conciliable encore avec l'éclipse d'Agathocle, et avec une correction *positive* séculaire du mouvement du nœud lunaire comprise entre $0''$ et $20''$ lorsqu'on ne veut pas admettre le passage d'Agathocle au sud de la Sicile.

Milan,
1880, *Décembre*.

On Mr. Stone's Alterations of Bessel's Refractions.

By W. H. M. Christie, Esq.

In a paper on Mean Refractions in the *Monthly Notices* for April 1880, Mr. Stone refers to some results which I gave in a paper on the Systematic Errors of the Greenwich N.P.D.'s, read at the meeting in January 1880, but not published till the middle of December last. As there would have been obvious inconvenience in following Mr. Stone in a discussion of an unpublished paper, I have preferred to defer any remarks on Mr. Stone's criticisms till the publication of my paper in the recently issued volume of the *Memoirs*. This has been the cause of the delay in my reply to Mr. Stone.

As regards the Greenwich circumpolar observations 1857 to 1865, Mr. Stone appears to be much concerned at the discordances between his residuals and mine; but the most important difference really admits of very simple explanation:—Mr. Stone had not stated what corrections he had applied, and he has not taken note of the corrections which I have applied, as specified in my paper. To begin with, Mr. Stone has not even allowed for the difference in our adopted co-latitudes, the corrections for which would increase my residuals by $0''\cdot20$. Then there is the still more important correction to the refractions beyond Z.D. 82°, which was applied to the Greenwich observations from the year 1857. Mr. Stone, it now appears, has brought back the results to Bessel's Refractions unaltered, whilst I have used the printed observations as affected with Main's correction. I certainly derived the impression from a perusal of Mr. Stone's paper of 1867, that he had taken no notice of this important correction, and I regret that there should have been this mistake on my part. Again, there is some doubt as to the exact limits of Mr. Stone's groups, for it so happens that as regards three of them, stars fall so near the boundary as to be easily transferred from one group to the next unless account is taken of the seconds of N.P.D. as well as of the degrees and minutes. In forming my residuals, as printed in the *Memoirs*, vol. xlv., I included θ *Ursæ Majoris* in the group 30° to 37° 40′, and f_1 *Cygni* in the group 37° 40′ to 43°. In both cases the precessional motion would carry the star, in the course of a few years, from one group to the next. Now, the transposition of the last-named star from the last group but one to the last group affects the last residual by $0''\cdot47$, which is more than twice the amount of Mr. Stone's estimate of the probable error of the result.

In order to give a fair comparison between Mr. Stone's residuals and my own, I have taken the results with the new law for R—D as given in my paper (*Memoirs*, vol. xlv. p. 165), and have applied a correction of $-0''\cdot20$ to reduce to co-latitude 38° 31′ 21″·80,

and a correction to the last residual to remove the effect of Main's correction beyond Z.D. 82°, viz. $\frac{8}{12} \times -1''{\cdot}06 = -0''{\cdot}71$ when f_1 *Cygni* is included in the last group, and $\frac{8}{9} \times -1''{\cdot}06 = -0''{\cdot}94$ when that star is transferred to the preceding group. I have then applied the differences between Mr. Stone's residuals A and B to reduce to Mr. Stone's diminished refractions and co-latitude 38° 31′ 21″·61. The comparison of Mr. Stone's work and mine stands thus :—

N.P.D. above Pole—N.P.D. below,

with Mr. Stone's Refractions, Co-lat. 38° 31′ 21″·61, *and new Law for R—D.*

N. P. D.		0°–15°	15°–30°	30°–37° 40′	37° 40′–43°	43°–45½°
Mr. Stone's values		−0″·15	+0″·10	+0″·14	+0″·08	−0″·10
Mr. Christie's „	I.	−0·37	+0·02	+0·10	−0·25	−0·14
„ „	II.	−0·37	+0·02	+0·10	−0·18	−0·61

In the residuals I. f_1 *Cygni* is included in the last group ; in II. it is transferred to the group 37° 40′–43°. These results of mine have been obtained by giving equal weight to each annual result, regardless of the number of observations. In the last group, four of the results have two observations S.P., and two or more above pole, and if double weight be given to these, the last residual of I. becomes − 0″·29.

Thus it appears that by simply applying the corrections for difference in adopted co-latitude and for Main's diminution in the refractions beyond Z.D. 82°, my last residual I. is brought into substantial agreement with Mr. Stone's, and, under these circumstances, I think I have some ground of complaint in regard to the language which Mr. Stone has used respecting the supposed discordance. His statement that either his results or mine are in error is based on a misapprehension, for which he is mainly responsible, in that he gave no explanation in his paper of 1867 of the corrections which he had applied to form his residuals and that he disregarded the explanation which I gave in presenting my residuals. I may add that I had no intention of casting any doubt on the accuracy of Mr. Stone's arithmetic, and that I did not start with any idea of examining his work. On discussing the circumpolar observations for the period 1851 to 1861 I arrived at results which gave so little support to Mr. Stone's diminution of Bessel's Refractions that I was naturally led to discuss the observations 1857 to 1865, which Mr. Stone had used,and finding that they did to a certain extent support his hypothesis, I thought it only fair to say so. But in giving a comparison between his results and mine, I stated that I did not know what corrections Mr. Stone had applied, and that the different system of weights which we had respectively used might perhaps account for a portion of the discordances. I

may remark that the difference in the R−D corrections with the old and new laws respectively affects the last residual by 0″·36, and that Mr. Stone had not given the slightest indication as to which formula he had adopted. If Mr. Stone had stated clearly in 1867 the corrections which he had applied to the Greenwich results, it might have been possible to verify his work. As it is, I must protest against the circumstance that discordances are still left between Mr. Stone's results and mine being taken as evidence of inaccuracy on either part. As regards my own work, I have, since the publication of Mr. Stone's paper, carefully recomputed the residuals, and have obtained identical results. The only changes I have made have been to transpose *β Ursæ Minoris* to the group 15°–30°,* *θ Ursæ Majoris* to the group 37° 40′–43°, and f_1 *Cygni* to the last group 43°–45½°.

I may add that I have not applied in this portion of my work corrections for error of the thermometer, as there is some uncertainty about the amount in individual years.

But, notwithstanding Mr. Stone's explicit statement in the *Monthly Notices* for April 1880, pp. 338, 339, of the corrections which he has applied in forming his last residual, there are several points to be cleared up:—

1. Corrections for errors of thermometer and barometer are stated to have been applied to the observations in 1857 and part of 1858. Now, I do not find in the Greenwich "Introductions" any observations in those two years which would fall in this group.

2. The limits of Mr. Stone's last group are N.P.D. 43° to 46° 29′. I have not been able to find any observations in the Greenwich "Introductions" between N.P.D. 45½° and 47½°.

3. The assumed co-latitude is stated to be 38°31′ 21″·80, but in his paper of 1867 Mr. Stone, after applying a correction —0″·205 obtains 38° 31′ 21″·61, which would imply an assumed co-latitude 38°31′21″·815. The difference is trifling, but it must be remembered that Mr. Stone gives his residuals to a thousandth of a second.

I call attention to (1) and (2), not in any spirit of criticism, but because there seems a possibility that as Mr. Stone extracted his results from the Greenwich Annual Catalogues, his last group may contain observations accidentally omitted from the Latitude Investigations in the Greenwich "Introductions" which I used.†

* In discussing the observations 1851–61 and 1862–65 I had taken 15½° as the limit of one of my groups, and had at first used the same limit for 1857–65. The correction only amounts to 0″·02.

† On examining the *Greenwich Observations* for 1857, I find that there are, as a matter of fact, several stars in the Annual Catalogue which, for some reason, are not included in the Latitude Investigation in the Introduction. Mr. Stone had not stated in his paper of 1867 that he had taken his results from the Annual Catalogues, and I thought it more natural that he should have taken them from the Introductions where they are collected. Thus our data are not the same.

On the rejection of the reflection observations by Mr. Stone I wish to offer a few remarks, not on account of its effect on the matter at issue, which is probably insignificant, but because it seems to be opposed to a principle usually accepted by astronomers—viz., that where an instrumental correction is determined by observations of a number of stars, the observation of each star may be used as a determination of its place, notwithstanding the fact that the observation in question has contributed *in a small degree* towards the determination of the instrumental correction on which the determination of place depends. Thus when the clock-error is deduced from transits of a number of clock-stars, the transit of each clock-star is used in connection with the clock-error to determine the R.A. of the star. Now, during the period 1857–1865 the zenith points were determined from observations extending over a fortnight, and including usually more than a dozen stars observed by reflection in each group. Thus the N.P.D. from the reflection observation of any one star would be virtually independent of that from the direct observation, since this star had only contributed in a small degree towards the determination of the zenith point. Again, for the R—D correction, two constants are determined by observations of a large number of stars, and the argument applies, therefore, with even greater force.

But, as regards the residuals, there is a point of some importance on which I am at direct issue with Mr. Stone—viz., the estimated probable error of the result for the last group. Mr. Stone obtains a weight of 28 or 24 with a probable error $0''\cdot2$, while with the same system of weights, I obtain a weight 6·5, with a probable error $0''\cdot4$. I have for the last group taken the probable error of an observation above the pole as $0''\cdot5$ and S.P. as $1''\cdot5$, and I have altogether 30 observations above pole and 17 below, which gives as an *inferior* limit to the probable error $0''\cdot38$, on the supposition that they all referred to the same star. It is just possible that the additional observations included by Mr. Stone may account for the discrepancy.

In any case there are, I believe, but few stars observed in this group; and where the same star—*Capella*, for instance—is observed every year on the same division of the circle, account ought to be taken of the probability of a systematic error * affecting the series. Thus the weight ought not to be assumed to be the sum of the weights for the separate years. Unless Mr. Stone has followed this last and erroneous system, I hardly see how he can have got such a large weight and small probable error for the result of his group.

I now pass on to the general question of Mr. Stone's proposed alterations in Bessel's Refractions.

* Such as division error or error of the micrometer screws. I have shown later on that this last error would affect the residual for *Capella* in the group 1868–1876 by $0''\cdot18$. And there may be unknown sources of error which would have a still larger effect.

First, as regards the value of the constant of refraction :—

I gather from Mr. Stone's paper on the whole that he gives up the diminution of the mean refractions which he advocated in 1867 ; but he accompanies this concession by criticisms directed against the validity of the evidence in favour of Bessel's constant of refraction.

In the first place, he brings forward his discussion of the observations 1857–1865, and appeals to his residuals, and to the accumulated evidence of the Greenwich circumpolar observations in favour of his diminution of the mean refractions ; while he calls attention to the very slight evidence on which Bessel's Refractions have been reintroduced. Now, it seems to me that Mr. Stone's alteration in the refractions was based on very inadequate data, and I cannot see any reason for his confining his attention to this particular period of nine years from 1857 to 1865. In his recent paper he has attempted to supplement it by a discussion of the period 1868–1876 ; but even then he only takes 18 years out of 42, and the last 9 years on which he relies do not lend much support to his views. So that it is really quite misleading to speak of the accumulated evidence of the Greenwich observations, when no account is taken of the 21 years of observation from 1836 to 1856 which tell against Mr. Stone's hypothesis. In the four periods, 1840–1847, 1851–1861, 1862–1865, and 1868–1876, which I have discussed, the first two support Bessel's Refractions, whilst the third and fourth show such large errors on either hypothesis that no certain conclusion can be drawn from them. The diagrams which I have given (*Memoirs*, vol. xlv. p. 161) show this at a glance much better than mere figures.

But Mr. Stone's hypothesis is undoubtedly supported by the circumpolar observations 1857–1865. His residuals for 1857–1865, with the diminished refractions, are :—

$$-0''.15 \qquad +0''.10 \qquad +0''.14 \qquad +0''.08 \qquad -0''.10;$$

or as I find them, with the law $b \sin z \cos^2 z$ for R—D,

$$-0''.37 \qquad +0''.02 \qquad +0''.10 \qquad -0''.25 \qquad -0''.14;$$

and with the law $b \sin z$,

$$-0''.33 \qquad +0''.20 \qquad +0''.37 \qquad +0''.07 \qquad +0''.22.$$

Against these we have to set the residuals, with Bessel's Refractions unaltered, for 1851–1861 :—

$$-0''.10 \quad +0''.02 \quad +0''.04 \quad +0''.27 \qquad +0''.15 \qquad +0''.04 \quad +0''.02 \quad -0''.02;$$

and for 1840–1847,

$$+0''.15 \quad +0''.05 \quad -0''.07 \quad -0''.18 \quad -0''.07 \quad +0''.02 \quad +0''.37 \quad +0''.20,$$

these last being taken in connection with Mr. Main's discussion of the observations below Z.D. 70° (*Memoirs*, vol. xxvi.).

These residuals compare favourably with Mr. Stone's, so that we have 19 years in favour of Bessel's Refractions, as against 9 years only in favour of Mr. Stone's alteration. And it is to be remarked that Mr. Stone has only formed five groups, thus simplifying the problem of obtaining small residuals. For with two quantities—viz., the co-latitude and the constant of refraction—at our disposal it is much easier to satisfy five equations than eight. But the mere circumstance that the residuals are small gives no indication by itself that the solution is deserving of confidence. In illustration of this, let us take the residuals from the Melbourne circumpolar observations 1863–1865, discussed by Mr. Stone in his paper of 1867. From these Mr. Stone deduces a correction of $+0''.15$ to the co-latitude, and of $-0.00372 \times$ refraction to Bessel's Refractions, and he adopts these without hesitation. But, after all, it appears that Bessel's Refraction undiminished, with Main's correction below Z.D. 82°, gives smaller residuals.

Mr. Stone's residuals for (S.P.D. above Pole — S.P.D. below) are :—

N.P.D.	180°–170°	170°–160°	160°–155°	155°–150°	150°–147°
	$-0''.23$	$+0''.20$	$+0''.44$	$-0''.03$	$-0''.39$;

while Bessel's Refractions, with correction $+0''.50$ to co-latitude, give :—

	$-0''.01$	$+0''.26$	$+0''.27$	$-0''.59$	$-0''.38$
Weight	122	26	8	8	11.

Multiplying the two sets of residuals by $\sqrt{(\text{Weight})}$, so that each has a probable error corresponding to weight, we have them in a form more fitted for comparison :—

Mr. Stone's Refractions	$-2''.54$	$+1''.02$	$+1''.25$	$-0''.08$	$-1''.29$;
Bessel's ,,	$-0''.11$	$+1''.33$	$+0''.76$	$-1''.67$	$-1''.26$.

Thus, on the whole, Bessel's Refractions give more accordant residuals, the greatest error corresponding to the algebraic sum of 1 observation above and 1 below pole, being $1''.67$ with Bessel's Refractions, and $2''.54$ with Mr. Stone's solution. This is also seen on applying the usual criterion of least squares, for the sums of $(\text{residual})^2 \times$ weight are $10''.72$ with Mr. Stone's

solution and only $6''\cdot72$ with mine. And yet *per se* Mr. Stone's residuals seem to leave little to be desired. If Mr. Stone had in this and in other cases given the probable error of his determination of x and y, the untrustworthiness of the solution would have been evident.

Somewhat similar criticism applies to the treatment adopted for the Greenwich circumpolar observations 1868–1876. The bulk of the observations are, as in the Melbourne results, included in the first two groups with vastly preponderating weight, and the correction to the refractions is sought to be established by means of two groups with small weights. The last group, $43°$–$46\frac{1}{2}°$, formed by Mr. Stone contains 11 stars with total weight 34, exactly equivalent as regards number of stars and weight to each one of the 36 groups of 11 stars given in the Introduction to the Nine-Year Catalogue, p. 13. Of these 36 groups 23 are lumped together by Mr. Stone in a single group with weight 930, while the "all-important" last group has, as just mentioned, a weight 34 only. The degree of accuracy to be expected in a residual with this weight may be seen by examining the means of groups with corresponding or greater weight in the Introduction to the Nine-Year Catalogue, or the dots which represent these means in the diagram given in my paper in the *Memoirs*. If dots corresponding to Mr. Stone's last two groups be inserted on the diagram the difficulty of determining any correction to the co-latitude or to the refractions from the 38 dots here laid down will, I think, be manifest. But as Mr. Stone has grouped the residuals, the accidental errors are, to a great extent, masked, and the omission of the weights leaves us without any indication of the relative values of the several groups. Thus, presenting only five groups, he, by means of an arbitrary correction to the co-latitude, obtains small residuals, to which he appeals in proof of the soundness of his hypothesis. The only satisfactory way to test this, however, is to compare it with the results given with Bessel's Refractions, applying, as I have done throughout, Main's correction below Z.D. 82°.

Thus we have, taking ten groups corresponding to those in my paper on the Greenwich N.P.D.'s. :—

N.P.D. above Pole—N.P.D. below, 1868–1876.

With Mr. Stone's diminished Refractions and Co-latitude 38° 31′ 21″·60.

$-0''\cdot21$ $+0''\cdot18$ $+0''\cdot28$ $+0''\cdot76$ $+0''\cdot28$ $+0''\cdot24$ $+0''\cdot28$ $+0''\cdot52$ $+0''\cdot72$ $+0''\cdot19$;

With Bessel's Refractions and Co-latitude 38° 31′ 21″·90,

$-0''\cdot10$ $+0''\cdot28$ $+0''\cdot35$ $+0''\cdot75$ $+0''\cdot12$ $-0''\cdot02$ $-0''\cdot07$ $-0''\cdot08$ $-0''\cdot34$ $-0''\cdot61$.

In the latter part of his paper (*Monthly Notices*, vol. xl. p. 346), Mr. Stone gives for the group N.P.D. 41°-46½° with the diminished refractions and co-latitude 38° 31′ 21″·60, the residual +0″·71. With Bessel's Refractions and the same co-latitude, he gets −1″·33. To this must be applied a correction of +0″·60 to reduce to co-latitude 38° 31′ 21″·90, and a further correction of +0″·60 on account of Main's correction to refractions below Z.D. 82°. Thus with Bessel's Refractions as corrected by Main, we have the small residual −0″·13 to set against Mr. Stone's residual +0″·71.

In this case the observations to which Main's correction is applicable have a total weight of about $\frac{4}{7}$ of that for the whole group. I have taken the mean value of Main's correction to this group as −1″·06 corresponding to mean N.P.D. 45° S.P. Thus for the three groups at large Z.D., Mr. Stone's diminution of Bessel's Mean Refractions gives somewhat larger residuals. Mr. Stone, of course, reduces his residuals by an appropriate correction to the co-latitude; but I would submit that this is not permissible. The annual results of the Greenwich observations show that the latitude has not changed in the course of more than 40 years, as Mr. Lewis Boss has pointed out in his memoir on the "Declinations of Stars" (p. 37),* and as may be seen from the results for co-latitude which I have collected in my paper (*Memoirs*, vol. xlv. pp. 156 and 167). So that we are not at liberty to assume one value for the observations 1857–1865, and another value for the observations 1868–1876.

As regards the observations of the Sun, I have fully discussed them for the whole period of 44 years, and I need not, therefore, further refer to the partial results which Mr. Stone adduces.

I now come to the comparison of the Greenwich observations with those at the Cape and Melbourne.

With respect to Melbourne, Mr. Stone, I think, does well to point out the difficulty as regards the large correction to the refractions north of the zenith, which he introduced (as he expresses it) "to force a close agreement between the results." But the influence of these on the determination of the refractions at Greenwich would have been clearer if he had given also the comparison with the Nine-Year Catalogue uncorrected. This comparison I now give, and it will readily be seen how far we are arguing in a circle :—

* I regret that this important work did not reach me till after my paper was read, so that I could not make reference to it as I might otherwise have done.

(*Corrected for Error of Thermometer at Greenwich.*)

N.P.D.	G'_6-M.	G_6-M.	G'_6-M'.	G_6-M'.
°	"	"	"	"
48	−0·37	−0·92	+2·33	+1·78
52	−0·25	−0·82	+1·67	+1·10
56	−0·04	−0·63	+1·41*	+0·82
60	+0·21	−0·41	+1·36	+0·74
64	+0·49	−0·16	+1·42	+0·77
68	+0·69	+0·01	+1·45	+0·77
72	+0·74	+0·03	+1·37	+0·66
76	+0·58	−0·16	+1·11	+0·37
80	+0·28	−0·50	+0·72	−0·06
84	+0·04	−0·78	+0·40	−0·42
88	+0·18	−0·69	+0·47	−0·40
92	+0·18	−0·74	+0·42	−0·50
96	+0·27	−0·71	+0·45	−0·53
100	+0·35	−0·72	+0·48	−0·59
104	+0·41	−0·77	+0·50	−0·68
108	+0·32	−0·99	+0·36	−0·95
112	+0·36	−1·16	+0·36	−1·16
116	+0·35	−1·51	+0·31	−1·55
120	+0·31	−2·15	+0·23	−2·23
122	+0·07	−1·84	−0·02	−1·93
124	+1·15	+0·33	+1·04	+0·22

G_6 = Greenwich Nine-Year Catalogue with Bessel's Refractions (1 − ·00532).
G'_6 = " " " with Bessel's Refractions.
M = Melbourne Catalogue 1870, with Stone's Corrections.
M′ = " " without " "

As Mr. Stone thinks he applied, in his paper of 1867, corrections for error of the thermometer in forming his residuals, though we have no direct evidence on the point, I have corrected the results for error of the thermometer at Greenwich (+0°·55) so as to put the case as favourably as possible for Mr. Stone. In my former paper I had not applied any such corrections to the Greenwich Catalogues, as there is some uncertainty with

* Mr. Stone gives this residual as 1″·65, which is apparently a misprint for 1″·40.

respect to the error of the thermometer in the case of three of them, and I should be inclined to say that Bessel's Refractions satisfy the Greenwich observations as they stand—*i.e.*, with an error of perhaps $+0°{\cdot}4$ or $+0°{\cdot}5$ in the thermometer.

I think it is clear that, whether we take the Melbourne N.P.D.'s, with or without Mr. Stone's corrections, the substitution of Bessel's Refractions for Mr. Stone's in the Greenwich Nine Year Catalogue greatly improves the residuals. And it is easy to distinguish between the effect of an alteration in the Greenwich refractions and the effect of an alteration in those at Melbourne. The former affects mainly the residuals at large N.P.D.'s, the latter those at small N.P.D.'s.

There can be no doubt that there is some systematic error in the Melbourne observations, increasing rapidly towards the north horizon,* and this must diminish our confidence in a comparison with this Catalogue. But it seems to me more probable that the discordances are due to some instrumental error than to a difference in the refractions north and south of the zenith. In the absence of detailed information on the flexure, R—D discordance, zenith points, and errors of the micrometer screws, it would be unsafe to suggest the cause or causes of systematic errors. It may be remembered that Professor Adams found a discordance amounting to $1''$ in the results for nadir point of the Cambridge Transit Circle, according as the instrument had been pointing to the North or the South, and this was ultimately traced to a defect in the soldering of one of the ends to the central cube. This may suggest that we ought to take account of the possibility of discontinuous as well as continuous functions in the expression of instrumental errors, and that a discordance in the nadir point does not necessarily imply a correction of the form $a \cos z$ in the flexure. At Greenwich such a discordance was found to result from error in the micrometer screws, and to be peculiar to the nadir point observation itself. Thus we are not absolutely driven to the improbable supposition of large differences in the refractive power of the atmosphere at different places and on different sides of the zenith. The principle of forcing an agreement between observations by arbitrary alterations of latitude and refractions without much regard to physical facts does not seem very philosophical. By such a process we temporarily get over a difficulty, but do not get any nearer to the truth.

I am quite ready to admit that the comparison with the Melbourne observations alone would not be conclusive, but the Cape results seem to leave no doubt as to the substantial accuracy of Bessel's constant of refraction, as the following comparison shows:—

* N.P.D. 48° corresponds to Z.D. 80° N. at Melbourne and to Z.D. 76° N. at the Cape.

Greenwich Nine-Year Catalogue—Cape Catalogue (1860).

(*Corrected for Error of Thermometer at Greenwich.*)

N. P. D.	Bessel's Refractions.	Bessel's Refractions (1 − ·00532).
°	″	″
48	+0·88	−1·46
52	+0·80	−1·28
56	+0·66	−1·26
60	+0·56	−1·27
64	+0·54	−1·22
68	+0·59	−1·12
72	+0 62	−1·06
76	+0·52	−1·14
80	+0·31	−1·35
84	+0·08	−1·58
88	+0·01	−1·66
92	+0·04	−1·65
96	+0·10	−1·62
100	+0·13	−1·66
104	+0·14	−1·73
108	+0·06	−1·92
112	+0·06	−2·10
116	−0·08	−2·56
120	−0·15	−3·21
122	−0·35	−2·85
124	+0·78	−0·62

In the second set of residuals the effect of using the diminished refractions at the Cape as well as at Greenwich is shown. A correction of $-0''{\cdot}58$ has been applied to the co-latitude of the Cape Observatory ($56^\circ\ 3'\ 56''{\cdot}44$) as representing the effect of the diminution in the refractions.

Thus in favour of Bessel's constant of refraction we have the circumpolar observations for nineteen years, the comparisons with the Cape and Melbourne, and the observations of the Sun for twenty-eight years; and in favour of Mr. Stone's diminution of the constant, circumpolar observations for nine years only, and observations of the Sun for fifteen years. And even for the limited period on which Mr. Stone relies, the residuals are largely dependent on the assumption of the law $b \sin z \cos^2 z$ for R–D. I have taken no account of the circumpolar observations in the periods 1862–1865 and 1868–1876, as their evidence is inconclusive.

Secondly, with respect to the law of refraction :—It may be taken, I think, that Bessel's formula for the decrease of temperature in the atmosphere is not consistent with fact, and that this is the reason why his refractions for zenith distances beyond 80° do not accord with observation. This being so, I cannot see that anything will be gained by tinkering up Bessel's theory and distributing the errors in a different manner. Mr. Stone may easily get a tolerable agreement for the limited range of observations which he discusses—viz., 80° to 85° Z.D. ; but his alteration would not satisfy a more extended series. Bessel's Refractions carry us very well as far as Z.D. 80°, and even perhaps to Z.D. 82°. Mr. Stone attempts to extend the range to Z.D. 85°, introducing, however, sensible discordances above Z.D. 80°. Is anything really gained by this? And how will observations below Z.D. 85° be satisfied? Bessel professed to satisfy the observed refraction at the horizon, and also secured a good agreement with observation as far as Z.D. 80° ; Mr. Stone throws over all refractions below Z.D. 85°, and introduces divergences from other theories as well as from observation between 75° and 80°. I am prepared to admit that Bessel's is an artificial theory, and that his law of refraction is erroneous, though the constant satisfies observations ; but there is a great convenience in retaining refractions which have been so generally used, until we have tables founded on a really satisfactory theory with which to replace them. Uniformity of system will greatly facilitate the determination of corrections. I have called attention in my former paper to the circumstance that Bessel's rate of decrease of temperature in the atmosphere is too small at small altitudes and too large at great altitudes. Laplace's hypothesis agrees much better with observations of temperature made during balloon ascents, and it makes the refraction at 85° smaller than Bessel's by 1″·93, which is fully two-thirds of the correction indicated by observation.

I do not propose to enter further on this question now. My reason for alluding to it is to point out that in order to secure an agreement at 85° there is no occasion to introduce comparatively large corrections to Bessel between 75° and 80° Z.D. The differences (Bessel—Laplace)* are only −0″·09 at 75° Z.D., and −0″·17 at 80° Z.D. ; while those for Bessel—Stone are +0″·14 and +0″·46. As the differences between Lubbock's Refractions and Bessel's are equally small, I think we may safely assume that at Z.D. 80° Bessel's theory is not in error by more than 0″·2, which only amounts to ·0006 × refraction, or about $\frac{1}{1600}$th part.

This point has an important bearing on the determination of

* Using Caillet's Tables (*Conn. des Temps*, 1851) calculated on Laplace's theory, and reducing to Bessel's constant of refraction and to temperature 50° F., I find the following differences (Bessel—Laplace) :—0″·00 at Z.D. 45° and 60° ; −0″·09 at 75° ; −0″·17 at 80° ; −0″·18 at 81° ; −0″·12 at 82° ; +0″·08 at 83° ; +0″·61 at 84° ; +1″·02 at 84½° ; +1″·93 at 85°.

the constant of refraction. For it is clear that if we want to find the correction y to this constant within such limits as ·003, an error of ·0006 in Bessel's theory is of no practical importance, and thus we extend our range of Z.D. to 80°, and double the effect on our residuals. Even Mr. Stone's discordance from Bessel's theory of +0″·46 would only imply an error of ·0015 × refraction; so that on his own showing there is no occasion to increase the difficulties of the problem by restricting the range to 75° Z.D.*

But the case is very different beyond 82° Z.D., where the discordances between different theories begin to increase rapidly in proportion to the refractions. It seems to me, then, that we may safely use Bessel's Refractions as far as Z.D. 80° for the determination of the constant of refraction, since the error of theory is but a small fraction of the uncertainty of the constant; and that beyond 82° Z.D., the conditions being reversed, observations may be used for determining the law of refraction, on the assumption that the constant is known with tolerable accuracy.

Thus for the determination of y from circumpolar stars, instead of Mr. Stone's limiting equations

$$x + 90y = a_1, \qquad x + 215y = a_m,$$

we have

$$x + 90y = a_1, \qquad x + 313y = a_n;$$

and from results at northern and southern Observatories, instead of

$$x + 116y = b_1, \qquad x + 230y = b_m,$$

we have

$$x + 116y = b_1, \qquad x + 316y = b_n;$$

and an error of ·003 in y will introduce into our residuals a_1 and a_n a difference of about 0″·65 instead of 0″·35.

I have taken Mr. Stone's limiting equations merely for the sake of comparison between the limits 75° and 80° Z.D. They are a trifle too favourable to the circumpolar observations, and perhaps hardly fair to those at northern and southern Observatories. We ought, I think, to assume some such equations as these:—

$$x + 92y = a_1, \qquad x + 313y = a_n,$$
$$x + 75y = b_1, \qquad x + 316y = b_n,$$

* I must admit that on the second page of my paper (*Memoirs*, vol. xlv. p. 152) I have spoken of Z.D. 75° as the limit at which the law of refraction begins to get somewhat uncertain, but on the next page and subsequently I have taken 78½°, and finally I have concluded that Bessel's Refractions are sensibly correct as far as Z.D. 80°. In explanation of these apparent inconsistencies, I may remark that the various theories of refraction *begin* to diverge about Z.D. 75°, but the divergence in the case of Bessel's, Laplace's, and Lubbock's Refractions is of little practical importance till beyond Z.D. 80°.

the sum of refractions for *Polaris* above and below pole being $92''$, and the refraction for a star at N.P.D. 81° equidistant from the zeniths of Greenwich and the Cape being $53''$. I have taken the coefficient of y as $53\sqrt{2}$, assuming the probable errors of the determinations of y and z (the correction to the constant of refraction for the Cape) to be the same. I do not, however, insist on this point, as the actual problem consists in the determination of *three* unknown quantities from a system of equations with different weights, and seems hardly to admit of statement in the simple form of two limiting equations with only two unknown quantities.

The greatest difference of residuals from circumpolar observations would be $221y$, whilst from northern and southern Observatories we should have $241y$.

These equations give, according to my view, a fair comparison of the relative value of circumpolar observations and of results at northern and southern Observatories for the determination of y, on the supposition that we are at liberty to assume different values of y for Greenwich and the Cape—an hypothesis which I am not prepared to admit without further evidence.

But, as I have shown in my paper on the Systematic Errors of the Greenwich N.P.D.'s, the case is far more favourable when we assume y the same at the two Observatories. For the satisfactory comparison, however, of observations at Greenwich and southern Observatories, we require a thorough discussion of the latitude, flexure, R−D correction and errors of micrometer screws for the Cape and Melbourne.* Reflection observations of stars extending over a large range of Z.D. are particularly wanted at Greenwich, as well as at the southern Observatories, so that the R−D correction may be determined from a curve, and not from an arbitrary law. We may hope in due course to have these data, and then we shall be able to make use under the most favourable conditions of the comparisons of the results at northern and southern observatories.

In discussing the circumpolar observations for the group Z.D. 80° to 85°, 1868–1876, Mr. Stone makes a remark (*Monthly Notices*, vol. xl. p. 345) which, I think, must have been written in momentary forgetfulness of the system which has been in use at Greenwich for upwards of 40 years, including the period during which he himself was there. I allude to the system of making the determinations virtually dependent on observations of stars alone. Mr. Stone says: "The co-latitude during the second period [1868–1876] appears to have been affected by the constant difference which existed during the period 1870–1874 between the nadir point determinations, and the virtual rejection of the observations with the reflecting eyepiece, by the application of constant corrections, to make their results agree in mean results

* In the *Monthly Notices* for 1879 December Mr. Stone has given the results of a discussion of the latitude of the Cape, which afford valuable information on this point, and we may hope for fuller details when the Cape Catalogue for 1880 is published.

with the zenith points found from the star observations. To some extent, however, any error which might arise from the course adopted will be compensated for when the co-latitude obtained from the observations themselves is adopted, as in the present comparison."

Now, it is well known that since 1836 the practice at Greenwich has been to deduce the R−D correction from observations of stars alone, and to include in it a constant term which represents the correction to adopted zenith points to make them agree, in mean results, with those found from the star observations. It is clear that it can make no difference in mean results for a year, whether we apply the correction at the end of the year to make the adopted zenith points agree with those resulting from stars, or whether we apply the requisite correction to each nadir point observation to make its results agree in the mean with those given by stars. The latter course was adopted in the years 1871–1874, and that it was virtually equivalent to the constant correction included in R−D in 1869 and 1870 is shown by the smallness of the constant term in R−D after the correction to the nadir point observation had been applied.

Mr. Stone's language would imply that the source of the discordance in the nadir point observation is unknown, and as its existence would then cast discredit on the results 1868–1876, it may be well to make a few remarks on it here, though I have already explained its origin in the errors of the micrometer screws in a former paper (*Monthly Notices*, vol. xxxvii. p. 18).

In 1868, at Mr. Stone's suggestion I believe, the practice was commenced of taking the nadir point at 0^r of the microscope micrometers, and thus was obtained that accordance of separate results which is so dear to some astronomers, but which is not always conducive to the elimination of systematic errors. The additional wear resulting from this increased use of one part of the screws gave rise to a discordance between the nadir observation and observations of stars which were taken at all parts of the screws. This discordance is shown in the years 1869 and 1870 by the large constant term in R−D; in subsequent years a correction was applied to each nadir observation derived from the mean results for the preceding year. In 1875, being dissatisfied with the large correction for runs virtually required to reduce the nadir observation to the mean of observations of stars which would fall at about $2^r{\cdot}5$, I suggested that the nadir observation should be taken at from $2^r{\cdot}5$ to $3^r{\cdot}5$, and it was then found that the correction to the nadir observation had changed from $-0''{\cdot}8$ to $+0''{\cdot}6$. This threw suspicion on the micrometer screws, and their errors were examined, as explained in my paper of 1876. The result was to show that corrections were required ranging from $-1''{\cdot}4$ at $\bar{1}^r{\cdot}3$ to $+0''{\cdot}8$ at $2^r{\cdot}3$, the correction at $0^r{\cdot}0$ being $-0''{\cdot}9$, corresponding very closely with the correction to the nadir observation $-0''{\cdot}8$. Further, observations taken for another purpose in 1868 showed that the errors of the micrometer

screws were sensibly the same at that time, with the exception of the portion about $0^{r}{\cdot}0$, which had suffered extra wear since 1868 from a preponderance of some 4000 observations taken at this part. On inspecting the threads of the screws they were found to be much worn, as indicated by the errors. Thus there is no mystery about the discordance of the nadir observation, and there is no need to assume a term $a \cos z$ in the flexure. The apparent change in the co-latitude on which Mr. Stone remarks has nothing to do with this correction, but is simply due to a change in the system of weights given to the close circumpolars and to the increased number of observations of wide circumpolars. It did not begin in 1871, when the correction to the nadir observation was first applied, but in 1872, when special attention was directed to the observation of wide circumpolars. Previous to that the large weight given to the four close circumpolars made the value of the co-latitude virtually depend on them, and if this system had been continued the same value would have been obtained in subsequent years. The four close circumpolars give for the period 1868–1876 38° 31′ 21″·95 with Bessel's Refractions, and 38° 31′ 21″·70 with the diminished refractions; whereas the stars about N.P.D. 22° give about 38° 31′ 21″·50 and 38° 31′ 21″·17. It is simply the admission of these latter stars to a fair share in the result which has produced an apparent change. This apparent change does not justify Mr. Stone in using two different co-latitudes in the two periods 1857–1865 and 1868–1876.

Mr. Stone has done good work in collecting the results for north refraction stars; but before making use of them it is important to know whether he has applied the corrections for errors of the micrometer screws. In the case of *Capella*, which has the largest weight of any star in the group, and which has been observed in every year, I find that the correction applicable on this account to Mr. Stone's "Excess of N.P.D. below Pole" amounts to $-0''{\cdot}18$, and the corrections for other stars may easily be larger. The necessity for similar corrections in the period 1857–1865 suggests the exercise of caution in accepting estimates of probable error based simply on the number of observations, even when the possibility of a small outstanding division error is included. The corrections to the residuals in 1874 for λ *Ursæ Minoris* (47 observations), *Polaris* (152 observations), *Cephei* 51 (60 observations), and δ *Ursæ Minoris* (57 observations), on account of error in the micrometer screws were $-0''{\cdot}11$, $+0''{\cdot}25$, $-0''{\cdot}52$, and $+0''{\cdot}39$ respectively. Corrections of such magnitude to the mean results of a large number of observations would hardly have been expected.

Blackheath,
1881, *January* 12.

Correction to Paper "On the Systematic Errors of the Greenwich North Polar Distances," *Memoirs*, vol. xlv. p. 156.

The Flexure correction for 1851 and 1852 in the table should be $+0''.73$ instead of $+0''.50$. The value adopted is not stated in the "Introductions" to the *Greenwich Observations*, and it was assumed to have been the same as for 1853; but on referring to the original records it appears that the value $+0''.73$ was used, being the mean of the determinations in December 1850 and February 1851, giving double weight to the latter.

Spectroscopic Results for the Motions of Stars in the Line of Sight, obtained at the Royal Observatory, Greenwich, IV.

(*Communicated by the Astronomer Royal.*)

The results for star motions here given are in continuation of those printed in the *Monthly Notices*, vol. xxxvi. p. 318, vol. xxxvii. p. 22, and vol. xxxviii. p. 493. The observations were made with the "half-prism" spectroscope, one "half-prism" giving a dispersion of about $18\frac{1}{2}°$ from A to H being used, except in a few cases of bright stars mentioned in the remarks, where a train of two "half-prisms," with a dispersion of 83° from A to H, was used. These dispersions have been inferred by measurement of the distance between b_1 and b_4 as compared with the wave-length measure. An eyepiece with a magnifying power of 14 was used. In some of the recent observations, since 1880 March, the pointer in the micrometer was coated with Balmain's luminous paint, forming a bright line with which the star-line and comparison line were respectively bisected. In other cases the field was illuminated by a luminous-painted diaphragm placed in the micrometer adapter. This phosphorescent illumination was found to be much superior to artificial light.

Motions of Stars in the Line of Sight in Miles per Second, observed with the Half-Prism Spectroscope.

(+ denotes Recession; − Approach.)

The initials W. C. and M. are those of Mr. Christie and Mr. Maunder respectively.

The weights are on a scale of $\frac{1}{2}$ to 5 for each observation.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				α Andromedæ.			
1879, Sept. 3	M.	6	F	− 8·5	−13·6	−20·5	
1880, Nov. 8	M.	6	F	+ 9·5	−37·0	−35·4	Misty. Spectrum steady though faint.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
			β Cassiopeiæ.				
1880, Oct. 1	M.	7	F	− 4·7	+32·4	+40·9	Spectrum steady though faint.
			γ Pegasi.				
1879, Sept. 3	M.	4	F	− 7·8	− 8·1	− 7·8	
1880, Nov. 17	W.C.	2	F	+13·8	−28·6	−20·7	
27	M.	12	F	+15·7	−61·6	−63·5	
			α Cassiopeiæ.				
1880, Feb. 21	M.	5	b_1	+11·5	− 0·4	+ 1·9	Interrupted by cloud.
Nov. 20	M.	4	b_1	+ 5·0	+37·1	+42·7	
			γ Cassiopeiæ.				
1880, Oct. 1	M.	9	F	− 6·6	−18·2	−23·0	Steady. Definition good.
Nov. 21	W.C.	3	F	+ 3·8	−55·3	−35·9	Definition bad.
			β Andromedæ.				
1878, Nov. 2	M.	9	b_1	+ 3·4	+ 9·6	+17·8	Definition good.
1880, Sept. 29	M.	9	b_1	− 6·0	+ 9·3	+ 8·9	Definition fair.
			β Arietis.				
1879, Sept. 3	M.	5	F	−17·7	−10·6	−12·5	Near the Moon. Faint.
1880, Nov. 17	W.C.	6	F	− 7·9	−20·1	−12·1	
			α Piscium.				
1880, Nov. 17	W.C.	1½	F	+ 8·7	−24·8	− 31·6	
27	M.	6	F	+11·5	−33·2	−43·4	Faint. Definition poor.
			γ¹ Andromedæ.				
1878, Nov. 2	M.	4	b_1	− 0·6	+ 3·9	+ 8·7	Definition good.
1880, Feb. 21	M.	6	b_1	+15·5	−34·4	−31·4	Definition fair.
			α Arietis.				
1878, Sept. 18	M.	3	b_1	−11·5	−18·1	−14·5	Definition poor.
Oct. 22	M.	4	b_1	− 2·1	−16·2	−22·1	Definition fair.
Nov. 2	M.	1	b_1	+ 1·4	− 1·7	− 1·4	Two-prism train.
1880, Nov. 20	M.	3	b_1	+ 7·3	−32·8	−33·8	

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				α Ceti.			
1878, Oct. 22	M.	2	b_1	− 4·1	−27·5	−30·4	Definition poor.
22	M.	2	b_4	− 4·1	−33·5	−43·1	b_4 not clearly isolated.
				β Persei.			
1880, Nov. 17	W.C.	3	F	+ 0·5	−24·1	−29·5	
				α Persei.			
1880, Nov. 8	M.	6	F	− 3·8	+26·2	+26·5	Misty. Steady but faint.
28	W.C.	1	F	+ 1·9	+11·7	+15·3	Sky hazy.
				ζ Persei.			
1880, Feb. 17	M.	1	b_1	+13·4	−40·4	−49·5	Star-lines faint.
				Aldebaran.			
1880, Jan. 26	M.	4	b_1	+16·0	+22·4	+22·9	Definition very poor.
				Capella.			
1880, Jan. 26	M.	6	b_1	+12·6	+17·7	+19·5	Definition poor.
				Rigel.			
1880, Jan. 30	M.	6	F	+13·3	+13·2	+ 7·3	Very tremulous. Direct comparison.
Mar. 13	M.	4	F	+15·7	+13·4	+12·8	
				γ Orionis.			
1880, Jan. 30	M.	8	F	+14·1	− 1·1	+ 0·3	
Mar. 17	M.	2	F	+17·5	− 5·7	− 7·1	Definition poor.
				β Tauri.			
1880, Jan. 30	M.	6	F	+14·3	− 8·7	− 9·7	Definition fair.
Mar. 15	M.	8	F	+18·5	−27·1	−29·1	Definition good.
				δ Orionis.			
1880, Jan. 30	M.	2	F	+13·2	− 2·0	− 0·8	Definition poor.
Mar. 13	M.	3	F	+17·0	−18·1	−15·7	Definition variable.
				ε Orionis.			
1880, Jan. 30	M.	6	F	+12·9	−37·6	−36·9	Definition fair.
Mar. 17	M.	4	F	+16·8	−31·9	−27·6	Spectrum tremulous.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				ζ Orionis.			
1880, Jan. 30	M.	3	F	+12·6	+ 7·3	+ 4·6	Definition poor.
Mar. 17	M.	2	F	+16·7	− 0·3	− 0·9	Definition poor.
				κ Orionis.			
1880, Jan. 30	M.	2	F	+11·4	+ 4·2	+ 3·6	Spectrum very tremulous.
Mar. 13	M.	2	F	+15·6	− 6·8	− 5·3	Spectrum very tremulous.
				α Orionis.			
1880, Jan. 26	M.	4	b_1	+11·5	+24·6	+20·6	Definition poor.
				β Aurigæ.			
1880, Mar. 13	M.	2	F	+17·2	−11·1	− 9·5	Definition fair.
Apr. 8	M.	8	F	+15·9	−12·1	−11·6	Definition very good.
				γ Geminorum.			
1880, Feb. 10	M.	1	F	+13·0	+18·8	+23·8	Observation unsatisfactory.
Mar. 15	M.	8	F	+18·1	− 2·8	+ 1·3	Definition good. Steady.
				Sirius.			
1880, Jan. 30	M.	8	F	+ 6·9	+28·2	+30·0	Definition poor.
30	M.	12	F	+ 6·9	+27·0	+22·6	Two-prism train.
Mar. 19	M.	8	F	+13·9	+ 1·3	+ 5·8	Two-prism train.
24	M.	9	F	+14·1	+ 1·6	+ 6·1	Two-prism train.
				β Canis Minoris.			
1880, Mar. 17	M.	3	F	+16·6	−14·4	−14·6	
				Castor.			
1880, Mar. 24	M.	4	F	+17·6	0·0	− 1·0	Two-prism train.
				Procyon.			
1880, Mar. 19	M.	8	F	+16·2	+ 3·0	+ 8·0	Two-prism train.
24	M.	10	F	+16·7	+ 7·1	+ 6·0	Two-prism train.
				Pollux.			
1880, Jan.				+ 4·5	−39·5	−43·1	Definition poor.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				α Hydræ.			
1880, Feb. 21	M.	3	b_1	+ 2·2	+35·6	+36·7	
				ϵ Leonis.			
1880, Feb. 17	M.	2	b_1	+ 3·1	− 5·1	− 3·1	
21	M.	3	b_1	+ 4·4	− 1·1	− 0·9	
				Regulus.			
1880, Mar. 17	M.	7	F	+ 9·1	+17·5	+15·6	Definition fair.
				γ^1 Leonis.			
1880, Feb. 17	M.	3	b_1	+10·3	−43·3	−40·2	Definition poor.
				β Ursæ Majoris.			
1879, May 21	M.	2	F	+12·3	+18·3	+ 5·2	Definition bad.
June 9	M.	5	F	+10·8	+49·2	+49·1	
1880, Apr. 8	M.	4	F	+11·4	0·0	+ 3·5	Definition fair.
May 22	M.	2	F	+12·2	+12·9	+11·7	Definition poor.
				α Ursæ Majoris.			
1880, Feb. 21	M.	2	b_1	+ 4·0	− 4·0	− 4·0	Definition bad. Very tremulous.
				δ Leonis.			
1880. Mar. 17	M.	8	F	+ 5·6	−38·4	−36·2	Definition good.
May 15	M.	6	F	+16·9	−15·1	−14·0	Definition fair.
				θ Leonis.			
1880, Mar. 17	M.	2	F	+ 5·0	+13·0	+ 9·3	Definition good.
				χ Ursæ Majoris.			
1879, May 21	M.	1	F	+13·4	+25·8	+43·6	Very tremulous. Doubtful.
				β Leonis.			
1880, Mar. 17	M.	4	F	+ 2·5	+10·2	+ 7·3	Definition poor.
				γ Ursæ Majoris.			
1879, May 21	M.	4	F	+12·2	+ 8·2	+ 6·3	Tremulous.
June 9	M.	6	F	+11·4	+23·5	+30·2	
1880. Apr. 8	M.	6	F	+ 9·7	+20·0	+18·4	
May 22	M.	2	F	+12·2	+ 4·3	+ 2·2	Definition poor. Faint.
June 28	M.	6	F	+ 9·4	+ 4·4	+11·8	Definition fair.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				δ Ursæ Majoris.			
1880, June 28	M.	2	F	+ 8·6	+20·8	+29·5	
				γ Virginis.			
1880, Mar. 17	M.	4	F	− 3·5	+57·4	+51·0	Definition fair.
May 1	M.	2	F	+ 9·8	+12·6	+16·5	
15	M.	1	F	+13·0	+27·4	+45·4	Definition poor.
				ε Ursæ Majoris.			
1879, May 21	M.	5	F	+10·4	+24·4	+51·2	
June 9	M.	4	F	+10·2	+ 9·8	+14·8	
1880, May 22	M.	4	F	+10·4	+ 0·2	+ 0·4	Definition poor.
June 4	M.	8	F	+10·3	+15·4	+20·9	Definition good.
				δ Virginis.			
1880, Feb. 26	M.	1	b_4	− 9·7	+62·7	+51·8	Definition poor.
				α Canum Venaticorum.			
1880, May 25	M.	8	F	+13·0	−30·2	−29·6	Definition good.
				ε Virginis.			
1880, Feb. 21	M.	2	b_1	−10·4	−28·9	−32·0	
26	M.	2	b_1	− 9·1	−26·7	−25·2	Definition poor.
				Spica.			
1880, May 1	M.	6	F	+ 6·1	+ 0·3	+ 1·8	Tremulous. Definition poor.
15	M.	6	F	+ 9·9	− 5·4	− 4·1	Very tremulous.
25	M.	4	F	+12·2	− 2·2	− 2·9	Very tremulous.
				ζ Ursæ Majoris.			
1880, June 4	M.	6	F	+ 9·9	+ 7·2	+11·4	Definition fair.
28	M.	4	F	+ 9·0	+35·1	+49·3	
				η Ursæ Majoris.			
1880, June 4	M.	6	F	+10·2	+14·2	+25·2	Definition good.
				Arcturus.			
1880, May 29	M.	7	b_4	+11·1	−24·6	−27·2	Definition bad.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				γ Boötis.			
1880, July 7	W.C.	2	F	+11·6	−50·5	−53·2	
				ϵ^2 Boötis.			
1880, Feb. 26	M.	4	b_1	−10·6	−14·3	− 8·3	Definition fair.
May 29	M.	2	b_1	+ 9·2	− 1·4	0·0	Definition bad.
				β Ursæ Minoris.			
1880, Feb. 21	M.	5	b_1	+ 2·0	+24·0	+19·7	Tremulous. Definition poor.
Sept. 13	M.	3	b_1	+ 3·9	+32·6	+31·6	Definition poor.
				β Libræ.			
1880, May 8	M.	12	F	+ 0·3	+ 7·4	+ 7·6	Definition good.
18	M.	4	F	+ 3·3	+12·4	+10·6	Tremulous. Definition poor.
July 13	W.C.	2	F	+15·8	+35·0	+31·0	Hazy. Spectrum tremulous.
				α Coronæ.			
1879, Sept. 3	M.	2	F	+11·2	+22·9	+22·0	
1880, May 18	M.	6	F	+ 3·9	+10·3	+ 8·5	Definition poor.
				α Serpentis.			
1880, Feb. 26	M.	3	b_4	−16·1	−30·5	−19·3	
				ϵ Serpentis.			
1880, May 8	M.	6	F	− 1·0	+44·5	+40·3	Definition good.
18	M.	4	F	+ 0·8	+25·8	+19·8	Definition poor.
				γ Herculis.			
1880, May 15	M.	1	F	− 0·5	−52·8	−82·3	Faint, but definition fair.
25	M.	1	F	+ 1·8	+ 7·8	+ 7·5	Definition bad. Very doubtful.
				η Draconis.			
1880, Sept. 13	M.	1	b_1	+ 1·3	+21·4	+20·4	Tremulous. Definition poor.
20	M.	4	b_1	+ 0·9	−27·2	−35·2	Definition fair.
29	M.	3½	b_1	− 0·3	+36·1	+39·2	Tremulous.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				β Herculis.			
1879, July 23	M.	1½	b_1	+11·5	+42·9	+41·6	Observation doubtful.
				ζ Herculis.			
1879, Aug. 22	M.	3	b_1	+10·7	+41·6	+30·2	Interrupted by cloud.
25	M.	4	b_1	+10·7	+18·0	+15·7	
				ζ Draconis.			
1882, Oct. 1	M.	2	F	+ 1·5	−23·7	−27·0	Faint but steady.
Nov. 8	M.	3	F	+ 1·2	−20·5	−18·6	Faint but steady.
				β Draconis.			
1880, Sept. 13	M.	2	b_1	+ 4·5	+40·5	+32·2	Tremulous. Definition poor.
27	M.	6	b_1	+ 4·2	+16·0	+18·7	Mist. Faint but steady.
				α Ophiuchi.			
880, July 15	M.	4	F	+ 7·9	−21·5	−19·6	Definition fair.
29	M.	6	F	+10·5	+14·9	+16·1	Definition fair.
Sept. 3	M.	1	F	+14·5	+25·7	+24·2	Definition bad.
				ξ Draconis.			
1880, Sept. 27	M.	1	b_1	+ 3·0	+ 8·3	+11·0	Observation doubtful.
				γ Draconis.			
1880, Sept. 13	M.	4	b_1	+ 4·7	−11·0	−11·4	Definition fair.
				α Lyræ.			
1880, July 29	M.	8	F	+ 3·4	−23·7	−25·2	Definition fair.
29	M.	8	F	+ 3·4	−15·8	−11·7	Two-prism train.
Sept. 3	M.	6	F	+ 7·3	−26·4	−28·2	Tremulous. Definition bad.
Nov. 27	M.	12	F	+ 5·5	−33·1	−40·0	
				γ Lyræ.			
1879, Sept. 3	M.	8	F	+ 8·0	+ 7·4	+ 6·7	Steady.
1880, Nov. 27	M.	6	F	+ 7·6	+ 8·6	+16·3	
				ζ Aquilæ.			
1880, July 15	M.	6	F	+ 1·4	−27·4	−23·8	Definition fair.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				δ Aquilæ.			
1880, July 15	M.	1	F	+ 0·5	+18·7	+17·4	Definition poor.
				β Cygni.			
1879, Aug. 25	M.	3	b_1	+ 6·4	−31·3	−38·5	Definition fair.
1880, Sept. 20	M.	3	b_1	+10·2	− 3·9	− 1·5	Interrupted by cloud and mist.
Nov. 20	M.	4	b_1	+10·8	−13·1	−22·3	
				γ Aquilæ.			
1879, Sept. 2	M.	1	b_1	+10·1	−49·3	−48·5	Mist. Definition poor.
				δ Cygni.			
1880, July 29	M.	2	F	− 1·0	−29·0	−27·6	
Sept. 3	M.	1	F	− 3·6	−22·8	−21·5	Faint. Definition poor.
Nov. 8	M.	10	F	− 8·1	− 6·0	− 6·6	Mist. Faint but steady.
				α Aquilæ.			
1880, July 20	M.	12	F	− 0·4	−10·5	−13·6	
				γ Cygni.			
1879, Sept. 3	M.	6	F	+ 3·0	+ 8·8	+ 5·3	
1880, Sept. 29	M.	4	b_1	− 6·9	− 5·2	− 6·9	Faint and tremulous.
				α Delphini.			
1879, Sept. 3	M.	2	F	+ 6·5	+21·3	+19·4	
1880, July 20	M.	1	F	− 4·4	+25·3	+24·3	Faint. Definition poor.
				α Cygni.			
1880, Sept. 29	M.	4	b_1	+ 5·1	−18·4	−20·5	Tremulous. Definition poor.
				ε Cygni.			
1878, Oct. 22	M.	5	b_1	+10·8	+14·8	+17·4	Tremulous.
				ζ Cygni.			
1880, Nov. 20	M.	1	b_1	−13·0	− 6·7	−18·8	Definition poor.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				α Cephei.			
1880, Nov. 21	W.C.	2	F	+ 5·1	−75·9	−48·6	
				β² Cephei.			
1880, Nov. 20	M.	3	b_1	− 2·7	+53·1	+53·8	Spectrum faint.
				ε Pegasi.			
1878, Sept. 18	M.	2	b_4	+ 7·2	−15·7	−14·6	Definition poor.
Oct. 22	M.	5	b_1	+14·8	−20·8	−20·5	Definition fair.
25	W.C.	2	b_1	+15·2	−23·5	−21·6	Two-prism train.
Nov. 2	W.C.	2	b_1	+16·3	+ 0·2	− 4·8	Two-prism train.
2	M.	1½	b_1	+16·3	−24·9	−23·5	Two-prism train.
1879, Aug. 25	M.	6	b_1	+ 0·6	+ 7·3	+14·2	Definition fair.
1880, Sept. 29	M.	2	b_1	−10·2	−17·2	−19·0	Faint and tremulous.
				η Pegasi.			
1879, Aug. 25	M.	4	b_1	− 5·4	−23·7	−23·5	Definition bad.
1880, Sept. 27	M.	1	b_1	+ 2·9	−10·3	−11·4	Faint. Doubtful.
				β Pegasi.			
1880, Sept. 27	M.	2	b_4	+ 2·1	+21·7	+23·2	Definition fair.
27	M.	2	b_1	+ 2·1	+ 8·6	+10·6	
				α Pegasi.			
1880, Sept. 3	M.	2	F	− 2·9	−21·5	−20·9	Definition bad.
Nov. 25	W.C.	3	F	+17·0	−77·1	−51·4	
27	M.	13	F	+17·2	−39·9	−50·0	

The following table gives the mean results of all the observations of star motions at Greenwich compared with the results obtained by Dr. Huggins. The probable errors of the Greenwich results, inferred from the separate discordances, are appended with the sign ±, in cases where there were sufficient observations to give a rough estimate of the probable error. The character of the line measured is given in the remarks, as on this depends very largely the accuracy of the observations.

Motions of Stars in the Line of Sight in Miles per Second.

+ denotes recession; − denotes approach. The weights are on a scale ½ to 5 for each observation.

Star's Name.	Concluded Motion.	Weight.	No. of Nights.	Dr. Huggins' Results.	Remarks.
α Andromedæ	−33 ± 4·2	28	9	−	F broad; nebulous.
β Cassiopeiæ	+?	7	1		F broad; diffused.
γ Pegasi	−46	18	3	−?	F diffused.
α Cassiopeiæ	+?	9	2		b_1
γ Cassiopeiæ	−27	12	2	+	F bright; nebulous.
β Andromedæ	+12	18	2		b_1 distinct.
β Arietis	−14	11	2		F broad; v. dark.
α Piscium	−36	7½	2		F narrow; sharp.
γ^1 Andromedæ	−?	10	2		b_1
α Arietis	−21	11	4		b_1 distinct.
α Ceti	−34?	4	1		b_1, b_4; fluted spectrum.
β Persei	−	3	1		F v. dark; sharp.
α Persei	+25	7	2		F faint; diffused.
ζ Persei	−	1	1		b_1 v. faint.
Aldebaran	+20 ± 2·0	26½	7	+?	b_1 v. distinct.
Capella	+27 ± 4·5	53	9	+	b_1 distinct; F sharp.
Rigel	+18 ± 1·9	40½	9	+15	F v. distinct.
γ Orionis	− 1 ± 1·4	25	4		F v. distinct.
β Tauri	−18	17	3		F broad; diffused.
δ Orionis	+ 4 ± 6·1	11½	6		F faint; narrow.
ε Orionis	−22 ± 10·3	23	6		F faint; narrow.
ζ Orionis	+ 9	7	3		F faint; narrow.
κ Orionis	− 1	4	2		F distinct.
α Orionis	+21 ± 1·8	54	8	+22	b_1, b_4; fluted spectrum.
β Aurigæ	− 7	12	3		F v. broad; neb.
γ Geminorum	+ 2	9	2		F broad; sharp.
Sirius	+20 ± 2·4	81	10	+18 to 22	F v. broad; diffused.
β Canis Minoris	−?	3	1		F broad; diffused.
Castor	+25 ± 4·2	34	8	+23 to 28	F v. broad; diffused.
Procyon	+24 ± 3·9	65	10	+	F broad; nebulous.
Pollux	−26 ± 4·0	74	13	−49	b_1 dist. F faint; sharp.
α Hydræ	+39	7½	2		b_1
ε Leonis	−14	8	3		b_1 distinct.
Regulus	+26 ± 2·4	48	8	+12 to 17	F broad.

Star's Name.	Concluded Motion.	Weight.	No. of Nights.	Dr. Huggins' Results.	Remarks.
γ^1 Leonis	$-38 \pm 4{\cdot}6$	25	5	—	b_1
β Ursæ Majoris	$+28 \pm 4{\cdot}7$	34	8	+17 to 21	F v. dark; brd.; neb.
α Ursæ Majoris	-27	6	2	−46 to 60	b_1; F faint.
δ Leonis	-28	14	2	+	F broad; dark.
θ Leonis	+?	2	1		F v. dark; brd.; diff.
χ Ursæ Majoris	+?	1	1		F v. dark; brd.; neb.
β Leonis	$+34$	18	4	+	F broad.
γ Ursæ Majoris	$+17 \pm 2{\cdot}7$	30	6	+17 to 21	F v. dark; brd.; neb.
δ Ursæ Majoris	$+10$	6	2	+17 to 21	F v. dark; brd.; neb.
γ Virginis	$+37$	10	4		F faint; diffused.
ϵ Ursæ Majoris	$+16 \pm 3{\cdot}7$	27	5	+17 to 21	F broad; diffused.
δ Virginis	+?	1	1		b_4; fluted spectrum.
α Canum Venat.	−?	8	1		F v. dark; broad; [sharp.
ϵ Virginis	-28	4	2		b_1
Spica	$+4 \pm 4{\cdot}0$	36	7	+	F narrow; sharp.
ζ Ursæ Majoris	$+18$	16	3	+17 to 21	F v. dark; brd; neb.
η Ursæ Majoris	-8	13	3	+	F broad; diffused.
η Boötis	−?	4	1		b_1 distinct.
Arcturus	$-33 \pm 2{\cdot}8$	110	14	−55	b_1 v. distinct; D_2 dark; [F sharp.
γ Boötis	−?	2	1		F broad.
ϵ^2 Boötis	$-7 \pm 3{\cdot}6$	36	7	—	b_1, b_4 distinct.
β Ursæ Minoris	$+26$	8	2		b_1 faint.
β Libræ	$+11$	18	3		F faint; sharp.
α Coronæ	$+40 \pm 4{\cdot}6$	35	7	+	F v. dark; brd; diff.
α Serpentis	−?	3	1		b_4; fluted spectrum.
ϵ Serpentis	$+35$	10	2		F broad.
γ Herculis	−?	2	2		F diffused.
η Draconis	$+3$	$8\frac{1}{2}$	3		b_1 v. faint.
β Herculis	−?	$4\frac{1}{2}$	2		b_1 v. faint.
ζ Herculis	$+25$	7	2		b_1
ζ Draconis	-22	5	2		F diffused.
α Herculis	-31	$5\frac{1}{2}$	2		b_4 fluted spectrum.
β Draconis	$+20$	$9\frac{1}{2}$	4		b_1, b_4 v. faint.
α Ophiuchi	$+17? \pm 7{\cdot}1$	38	9		F v. diffused.
ξ Draconis	+?	1	1		b_1 v. faint.
γ Draconis	$-17 \pm 2{\cdot}0$	$18\frac{1}{2}$	5		b_1, b_4 v. distinct.
α Lyræ	$-34 \pm 2{\cdot}4$	126	14	−44 to 54	F v. dark; brd.; diff.

Star's Name.	Concluded Motion.	Weight.	No. of Nights.	Dr. Huggins' Results.	Remarks.
γ Lyræ	+ 9	14	2		F broad; diffused.
ζ Aquilæ	−16?±16·5	27½	5		F v. broad; diffused.
δ Aquilæ	−?	1	1		F broad.
β Cygni	−18	10	3		b_1 distinct.
γ Aquilæ	−14	7	3		b_1 distinct.
δ Cygni	−17 ± 3·6	25	5		F diffused.
α Aquilæ	−13?±11·8	21	9		F v. broad; diffused.
γ Cygni	−15 ± 3·4	37	9	−	b_1, b_4 dist.; F sharp.
α Delphini	+22	3	2		F v. broad.
α Cygni	−43 ± 4·4	51	9	−39	b_1 faint; F sharp.
ϵ Cygni	+18	12½	4		b_1, b_4
ζ Cygni	−?	1	1		b_1 indistinct.
α Cephei	−?	2	1		F v. dark; sharp.
β^2 Cephei	+?	3	1		b_1 indistinct.
ϵ Pegasi	−15 ± 2·5	32	10		b_1, b_2, b_4 distinct.
η Pegasi	−21	5	2		b_1
Fomalhaut	?	9	2		F distinct.
β Pegasi	+19	14	3		b_1, b_4; fluted spectrum.
α Pegasi	−34 ± 5·9	34	7	−	F broad; distinct.

Royal Observatory, Greenwich,
1881, *January* 13.

Observations of Occultations of Stars by the Moon, of Phenomena of Jupiter's Satellites, and of the Eclipse of the Sun, 1880, December 31, made at the Royal Observatory, Greenwich, in the Year 1880.

(*Communicated by the Astronomer Royal.*)

Occultations of Stars by the Moon.

Day of Obs.		Phenomenon.	Telescope.	Power.	Moon's Limb.	Mean Solar Time of Obs. h. m. s.	Observer.
1880, Jan.	20	(*a*) Disapp. of ϵ Arietis	E. Eq.	140	Dark	7 4 27·7	A. D.
March	13	Disapp. of 101 Piscium	,,	,,	,,	7 34 36·6	C.
	21	Disapp. of d^2 Cancri	,,	,,	,,	7 16 25·6	,,
	24	(*b*) Disapp. of p^2 Leonis	,,	,,	,,	13 35 54·4	,,
April	16	Disapp. of 56 Geminorum	Altaz.	100	,,	10 43 0·9	T.
May	18	(*c*) Disapp. of p^2 Leonis	E. Eq.	140	,,	7 56 58·1	C.
Sept.	13	Disapp. of 50 Sagittarii	Altaz.	100	,,	7 14 51·5	,,
Nov.	17	(*d*) Disapp. of υ' Tauri	,,	,,	Bright	6 54 39·9	,,
		Disapp. of υ' Tauri	E. Eq.	140	,,	6 54 39·4	A. P.
	19	Disapp. of η Geminorum	,,	,,	,,	8 52 4·3	C.
		(*e*) Reapp. of η Geminorum	,,	,,	Dark	9 58 12·5	,,
		Disapp. of μ Geminorum	,,	,,	Bright	13 39 40·3	,,
		Reapp. of μ Geminorum	,,	,,	Dark	14 43 10·3	,,
	20	Disapp. of ζ Geminorum	Altaz.	100	Bright	8 16 40·8	J. P.
		Reapp. of ζ Geminorum	,,	,,	Dark	9 14 31·4	,,

Notes.

(*a*) The star seemed to disappear gradually.
(*b*) Images very bad; observation doubtful, as the observer lost sight of the star several times before it finally disappeared.
(*c*) Observation satisfactory.
(*d*) The observer lost sight of the star once or twice when close to the limb.
(*e*) The reappearance was not sudden; the star was first seen as a very small spot, and in about 0^s·2 or 0^s·3 it flashed out to its full brightness.

Phenomena of Jupiter's Satellites.

Day of Observation.	Satellite.	Phenomenon.	Telescope.	Power.	Mean Solar Time of Observation.	Mean Solar Time of N.A.	Observer.
					h m s	h m s	
1880, Aug. 2	I.	Occ. reapp. First seen	E. Eq.	140	12 44 9	12 45	C.
		Last contact	,,	,,	12 45 22		
2	(*a*) III.	Tr. egr. Bisection	,,	,,	12 59 10	13 6	,,
		Last contact	,,	,,	13 2 24		
9	III.	Tr. egr. First contact	,,	,,	14 32 57	14 34	J.
		Bisection	,,	,,	14 39 26		
		Last contact	,,	,,	14 46 15		
9	I.	Occ. reapp. First contact	,,	,,	14 33 57	14 34	,,
		Bisection	,,	,,	14 34 52		
		Last contact	,,	,,	14 35 56		
31	II.	Occ. reapp. Bisection	,,	,,	12 55 51	12 58	T.
		Last contact	,,	,,	12 58 20		
Sept. 1	I.	Ecl. disapp. First obscuration	,,	310	11 18 41	11 20 41	A. D.
		Last seen	,,	,,	11 20 43		
2	I.	Tr. ingr. First contact	,,	,,	9 27 49	9 29	C.
		Bisection	,,	,,	9 30 3		
		Last contact	,,	,,	9 32 3		

Day of Observation.	Satellite.	Phenomenon.	Telescope.	Power.	Mean Solar Time of Observation. h m s	Mean Solar Time of N. A. h m s	Observer.
Sept. 2	I.	Tr. egr. First contact	E. Eq.	310	11 35 43		
		Bisection	,,	,,	11 38 27	11 42	C.
		Last contact	,,	,,	11 40 27		
3	III.	Ecl. disapp. Last seen	,,	,,	11 17 24	11 17 25	T.
	(*b*) III.	Ecl. reapp. First seen	,,	,,	13 44 29		
		Full brightness	,,	,,	13 45 56	13 47 36	,,
7	II.	Ecl. disapp. Last seen	,,	,,	11 11 54	11 11 1	H. P.
16	(*c*) II.	Tr. ingr. First contact	,,	,,	9 10 57		
		Last contact	,,	,,	9 16 18	9 12	A. D.
Oct. 2	(*d*) I.	Tr. ingr. First contact	,,	140	10 48 34		
		Last contact	,,	,,	10 54 18	10 51	,,
2	(*e*) II.	Occ. reapp. First contact	,,	,,	11 1 31		
		Last contact	,,	,,	11 8 20	11 5	,,
2	(*f*) I.	Tr. egr. First contact	,,	,,	13 0 12		
		Last contact	,,	,,	13 5 6	13 4	,,
11	(*g*) I.	Tr. ingr. First contact	,,	,,	5 59 30		
		Last contact	,,	,,	7 3 21	7 0	,,
11	(*h*) II.	Tr. egr. First contact	,,	,,	7 39 59		
		Last contact	,,	,,	7 44 33	7 46	,,
11	(*i*) I.	Tr. egr. First contact	,,	,,	9 8 39		
		Last contact	,,	,,	9 13 8	9 13	,,

Day of Observation.	Satellite.	Phenomenon.	Telescope.	Power.	Mean Solar Time of Observation.	Mean Solar Time of N. A.	Observer.
					h m s	h m s	
Oct. 18	(*k*) II.	Tr. ingr. First contact	E. Eq.	140	7 24 9		
		Bisection	,,	,,	7 25 39	7 25	C.
		Last contact	,,	,,	7 27 38		
18	I.	Tr. ingr. First contact	,,	,,	8 43 56		
		Bisection	,,	,,	8 45 26	8 44	,,
		Last contact	,,	,,	8 47 25		
18	(*l*) II.	Tr. egr. First contact	,,	,,	9 54 59		
		Bisection	,,	,,	9 57 44	10 2	,,
		Last contact	,,	,,	10 0 13		
18	I.	Tr. egr. Last contact	,,	,,	10 56 4	10 57	,,
25	(*m*) I.	Tr. egr. First contact	S. E. Eq.	130	12 36 3		
		Bisection	,,	,,	12 40 12	12 41	M.
		Last contact	,,	,,	12 44 11		
Nov. 2	I.	Ecl. reapp. First seen	E. Eq.	140	12 18 3	12 17 50	J.
3	II.	Occ. disapp. First contact	Naylor	215	6 28 45		
		Bisection	,,	,,	6 31 15	6 30	W. C.
		Last contact	,,	,,	6 33 15		
3	II.	Occ. disapp. First contact	E. Eq.	310	6 30 25		
		Bisection	,,	,,	6 31 55	6 30	C.
		Last contact	,,	,,	6 33 24		

Day of Observation.	Satellite.	Phenomenon.	Telescope.	Power.	Mean Sola Time of Observation. h m s	Mean Solar Time of N. A. h m s	Observer.
Nov. 3	(*n*) I.	Tr. ingr. First contact	Naylor	215	6 39 44	6 39	W. C.
		Bisection	,,	,,	6 41 3		
		Last contact	,,	,,	6 43 13		
3	I.	Tr. ingr. First contact	E. Eq.	310	6 38 38	6 39	C.
		Bisection	,,	,,	6 40 53		
		Last contact	,,	,,	6 42 38		
3	III.	Tr. ingr. First contact	Naylor	215	6 54 30	6 54	W. C.
		Bisection	,,	,,	6 59 39		
		Last contact	,,	,,	7 3 29		
3	(*o*) III.	Tr. ingr. First contact	E. Eq.	310	6 52 51	6 54	C.
		Bisection	,,	,,	6 58 50		
		Last contact	,,	,,	7 6 4		
3	I.	Tr. egr. First contact	,,	,,	8 47 32	8 52	,,
		Last contact	,,	,,	8 51 47		
3	III.	Tr. egr. Bisection	Naylor	215	9 14 41	9 21	W. C.
		Last contact	,,	,,	9 21 40		
3	III.	Tr. egr. Bisection	E. Eq.	310	9 15 58	9 21	C.
		Last contact	,,	,,	9 20 42		
3	(*p*) II.	Ecl. reapp. First seen	,,	,,	10 27 49	10 28 43	,,

Day of Observation.	Satellite.	Phenomenon.	Telescope.	Power.	Mean Solar Time of Observation.	Mean Solar Time of N. A.	Observer.
					h m s	h m s	
Nov. 4	(*q*) I.	Ecl. reapp. First seen	S. E. Eq.	130	6 46 18	6 46 42	M.
	I.	Ecl. reapp. First seen	E. Eq.	310	6 45 57	6 46 42	T.
		Full brightness	,,	,,	6 47 27		
10	I.	Tr. ingr. First contact	Naylor	215	8 23 19		
		Bisection	,,	,,	8 25 29	8 25	M.
		Last contact	,,	,,	8 27 48		
10	II.	Occ. disapp. First contact	,,	,,	8 46 34		
		Bisection	,,	,,	8 48 44	8 47	,,
		Last contact	,,	,,	8 50 9		
19	(*r*) I.	Tr. egr. Last contact	E. Eq.	310	6 53 26	6 53	C.
19	II.	Tr. egr. First contact	,,	,,	8 27 11		
		Bisection	,,	,,	8 29 11	8 34	,,
		Last contact	,,	,,	8 30 40		
21	(*s*) II	Ecl. reapp. First seen	Naylor	110	4 56 34	4 58 4	W. C.
		Full brightness	,,	,,	4 59 3		
21	III.	Occ. reapp. Last contact	,,	215	5 48 22	5 57	,,
21	III.	Ecl. disapp. First obscuration	,,	,,	7 36 0	7 43 23	,,
	(*t*) III.	Last seen	,,	,,	7 44 4		

Day of Observation.	Satellite.	Phenomenon.	Telescope	Power.	Mean Solar Time of Observation. h m s	Mean Solar Time of N. A. h m s	Observer.
Nov. 21	(*u*) III.	Ecl. reapp. First seen	,,	,,	9 53 23	9 58 24	W. C.
		Full brightness	,,	,,	10 1 2		
21	III.	Ecl. reapp. First seen	E. Eq.	310	9 53 12	9 58 24	C.
		Full brightness	,,	,,	9 59 5		
27	I.	Ecl. reapp. First seen	,,	,,	7 2 39	7 2 56	,,
27	I.	Ecl. reapp. First seen	Naylor	215	7 2 26	7 2 56	M.
28	(*v*) III.	Occ. disapp. First contact	,,	,,	6 49 2	6 58	W. C.
		Bisection	,,	,,	6 53 12		
		Last contact	,,	,,	7 1 20		
28	II.	Ecl. reapp. First seen	,,	,,	7 32 33	7 34 3	,,
		Full brightness	,,	,,	7 36 3		
28	(*w*) III.	Occ. reapp. First seen	,,	,,	9 25 11	9 35	,,
		Last contact	,,	,,	9 30 40		
Dec. 3	(*x*) I.	Tr. egr. First contact	E. Eq.	140	10 27 55	10 31	A. D.
		Last contact	,,	,,	10 33 25		
3	(*y*) II.	Tr. ingr. First contact	,,	,,	10 39 54	10 47	,,
		Last contact	,,	,,	10 48 37		
Dec. 20	(*z*) I.	Ecl. reapp. First seen	Altaz.	100	7 20 31	7 19 32	C.
30	II.	Ecl. reapp. First seen	E. Eq.	140	7 17 46	7 17 51	J.

Notes.

(*a*) Images very blurred and unsteady.

(*b*) The planet and satellites were ill-defined.

(*c*) Limb ill-defined.

(*d*) The images were diffused and tremulous and the observation difficult. The satellite could be traced for a quarter of an hour on the disk of Jupiter, and was distinctly brighter than the surface of the planet near the limb.

(*e*) Images diffused and tremulous.

(*f*) Tremulous; light cloud.

(*g* and *h*) Ill-defined and tremulous.

(*i*) Steady, but diffused.

(*k*) The satellite when on the disk of Jupiter appeared brighter than the surrounding surface.

(*l*) The shadows of the satellites were intensely black.

(*m*) Limb of planet tremulous; light clouds passing.

(*n*) The satellite was much brighter than Jupiter's disk near the limb. About eleven minutes after the last contact it was invisible on the planet.

(*o*) The satellite was seen bright on Jupiter's disk about seven minutes after the last contact, and barely visible eleven minutes later.

(*p*) Definition very bad.

(*q*) There was a considerable amount of fog, which probably prevented the first speck of light being recognised at once. The images were very steady.

(*r*) Not good; very cloudy.

(*s*) and (*t*) Observation good.

(*u*) A few seconds late.

(*v*) Sky hazy; definition fair. The satellite was seen very distinctly through the edge of the planet's disk.

(*w*) Definition bad.

(*x*) The satellite faint; the sky thick.

(*y*) The satellite faint; observation difficult.

(*z*) Planet faint on account of fog; the observation not satisfactory.

Eclipse of the Sun, 1880, *December* 31.

Beginning and Ending of the Eclipse.

	Phenomenon.	Telescope.	Power.	Mean Solar Time of Observation.	Observer.
				h m s	
1880, Dec. 31 (*a*)	First Contact	S.E. Eq.	295	1 40 33·6	W. C.
,, (*b*)	,,	E. Eq.	70	1 40 32·9	D.
,,	,,	Naylor	215	1 40 35·8	A. D.
,, (*c*)	,,	Altazimuth	66	1 40 37·4	T.
,, (*d*)	Last Contact	S.E. Eq.	295	3 28 23·0	W. C.
,, (*e*)	,,	E. Eq.	140	3 28 19·3	C.

Notes.

(*a*) Probably a few seconds late.

(*b*) At the time of contact the Sun's limb was steady and free from cloud. Clouds were, however, passing over the Sun till about 20 seconds before first contact, occasionally obscuring the limb.

(*c*) Clouds only just left the sun a few seconds before first contact.

(*d*) Observation pretty good.

(*e*) Very tremulous.

During the Eclipse the following observations were made with the S. E. Equatoreal:—

h m h m From 1 40 to 1 55 G.M.T.,	10 differences of N.P.D. of N. and S. cusps.
1 55 to 2 15	10 differences of R.A. of p and f cusps.
2 15 to 2 40	13 differences of N.P.D. of S limbs of Sun and Moon.
2 40 to 3 0	11 differences of N.P.D. of p and f cusps.
3 0 to 3 25	12 differences of R.A. of p and f cusps.

The clear aperture of the object-glass of the S.E. Equatoreal is $12\frac{3}{4}$ inches, of the East Equatoreal $6\frac{3}{4}$ inches, of the Altazimuth 4 inches, and of the Naylor Equatoreal 6 inches.

The initials W. C., D., C., A. D., M., T., H. P., J. P., J., and A. P., are those of Mr. Christie, Mr. Dunkin, Mr. Criswick, Mr. Downing, Mr. Maunder, Mr. Thackeray, Mr. H. Pead, Mr. Power, Mr. James, and Mr. A. Pead.

Royal Observatory, Greenwich,
1881, *January* 5.

Occultations of Stars by the Moon seen at the Stonyhurst Observatory. By the Rev. S. J. Perry.

		Phenomenon.	Moon's Limb.	G. M. T. h m s	Observer.	Remarks.
1879, Dec.	22	Disapp. of 101 Pisc.	dark	5 3 35·0	S. J. P.	Instantaneous. Double Lunar burr.
	25	„ „ 36 Tauri	„	6 0 44·9	W. C.	Good. Instantaneous.
	26	„ „ κ Tauri		6 29 35·1	S. J. P.	Obsn. very difficult. Star very faint.
		Reapp. „ „		7 24 39·4	„	Rather late.
1880, Jan.	20	Disapp. „ ϵ Arietis	dark	7 4 43·9	W. C.	Very good. Instantaneous.
		Reapp. „ „	bright	8 23 50·6	„	Good. Instantaneous.
	28	Disapp. „ B.A.C. 3407	„	10 7 21·9	S. J. P.	Difficult to see when close to Moon.
		Reapp. „ „	dark	11 10 29·6	„	Star rather faint, perhaps somewhat late. Haze.
	28	Disapp. „ π Leonis	bright	11 29 8·8	„	Good. Limb unsteady.
		Reapp. „ „	dark	12 31 47·4	„	Excellent.
March	13	Disapp. „ 101 Pisc.	„	7 29 54·4	W. C.	Excellent. Dark limb very distinct. Star appeared to grow slightly fainter about 0ˢ·5 before disappearance.
	17	Disapp. „ 103 Tauri	„	12 27 31·7	„	Very good, slight haze round Moon.
	18	„ „ 132 Tauri	„	6 12 23·9	„	Very good. Instantaneous.
		Reapp. „ „	bright	7 16 3·9	„	Excellent.
	21	Disapp. „ d^2 Cancri	dark	7 15 33·5	S. J. P.	Instantaneous.
		Reapp. „ „	bright	8 14 46·9	„	Good. Star faint.
	25	Disapp. „ B.A.C. 4006	dark	11 4 51·9	W. C.	Very good. Moon nearly full.
		Reapp. „ „		12 3 26·6	„	Nearly 1ˢ late.

		Phenomenon.	Moon's Limb.	G. M. T. h m s	Observer.	Remarks.
April	14	Disapp. „ 121 Tauri	dark	8 32 27·8	W. C.	Sky hazy, thin clouds passing. Good.
	16	„ „ 56 Geminorum	„	10 37 52·5	„	Instantaneous. Defn. very good. Limb visible.
		Reapp. „ „	bright	11 21 46·8	„	Very good.
May	16	Disapp. „ *h* Leonis	dark	10 42 3·6	S. J. P.	Very good. Instantaneous.
Sept.	13	„ „ 50 Sagitt.	„	7 10 49·7	W. C.	Good.
		Reapp. „ „	bright	8 5 5·5	„	Very good.
Oct.	10	Disapp. „ B.A.C. 6561	„	8 23 15·6	S. J. P.	Approx. Fair. } Passed terminator 3^m $37^s·5$ bef. disapp.
		Reapp. „ „	„	8 32 54·1	„	Defn. poor, unsteady. }
	19	Disapp. „ ζ Arietis	„	17 1 50·3	W. C.	Screen used. Well seen right up to limb. Instantaneous.
	20	„ „ 32 Tauri	„	10 27 42·2	„	Screen used. Very good.
		Reapp. „ „	dark	11 30 48·7	„	Screen used. Very good.
Nov.	19	Disapp. „ η Geminorum	bright	8 54 34·9	„	Excellent. Screen used.
		Reapp. „ „	dark	9 59 59·4	„	Excellent. Screen used.
		Disapp. „ μ Geminorum	bright	13 41 6·5	„	Excellent. Screen used.
	20	„ „ ζ^2 Geminorum	„	8 19 49·0	„	Sky slightly hazy, but observation very good.
		Reapp. „ „	dark	9 18 31·1	„	Very good, screen used. Sky improving.
		„ „ ζ^1 Geminorum	„	9 18 7·4	„	Very good.
	21	Disapp. „ 3 Cancri	bright	12 34 22·5	„	Difficult, owing to passing clouds.
		Reapp. „ „	dark	13 40 33·7	„	Excellent.

Observations of Jupiter's Satellites made at the Stonyhurst Observatory. By the Rev. S. J. Perry.

	Satellite.	Phenomenon.	G. M. T. h m s	In excess of N. A. m s	Observer.	Remarks.
1879, Dec. 12	II.	Oc. D. First contact	4 56 5·6		W. McK.	Defin. good at times. Thin clouds passing.
		Bisection	58 10·2			
		Last seen	5 0 32·5			
23	IV.	Sh. E. Int. contact	6 38 14·4		S. J. P.	Limb boiling. Defin. fair at centre. Sh. very black at edge of ♃. Obs. indifferent.
		Bisection	49 57·0			
	I.	Oc. D. Ext. contact	7 47 32·4		,,	Defin. very bad.
		Bisection	49 57·0			
24	I.	Tr. I. Ext. contact	4 57 28·5		W. McK.	At bisection thin clouds; at internal contact sky very clear.
		Bisection	5 0 41·1			
		Int. contact	4 1·9			
	I.	Sh. I. Bisection	6 16 19·7		W. C.	Hazy and slightly tremulous. Sh. very black.
		Int. contact	17 37·2			
	I.	Tr. E. Bisection	7 20 8·3		,,	Satellite exceedingly faint on planet. Unsteady.
		Ext. contact	22 21·3			
1880, Jan. 22	II.	Tr. E. Bisection	6 15 38·7		,,	Satellite very faint off disk. Unsteady.
		Ext. contact	17 39·8			
Sept. 8	I.	Ec. D. Last seen	13 16 10·3	+0 52·3	,,	Very good.
28	III.	Sh. I. Bisection	13 33 29·2			Good. Sh. very black, but very small.
		Int. contact	35 50·7			

	Satellite.	Phenomenon.	G. M. T. h m s	In excess of N. A. m s	Observer.	Remarks.
Sept. 28	,,	Tr. I. Ext. contact	14 34 27·9		W. C.	Sky very misty, defin. rather poor. Sh. still
		Bisection	38 54·4			very black.
		Int. contact	44 44·4			
28	,,	Sh. E. Int. contact	15 59 52·5		,,	Sky hazy, but defin. fairly good.
		Bisection	16 3 9·0			
		Last seen	7 11·5			
Oct. 2	II.	Ec. D. Last seen	8 14 45·7	+0 37·7		Rather difficult owing to brilliancy of planet.
2	I.	Sh. I. Bisection	10 45 30·6		,,	Rather faint. Defin. good.
		Int. contact	47 35·4			
2	,,	Tr. I. Ext. contact	10 51 7·0		,,	Hazy. Defin. rather poor.
		Bisection	52 31·7			
		Int. contact	54 52·4			
2	,,	Tr. E. Bisection	13 1 40·5		,,	Hazy. Defin. poor.
		Ext. contact	4 11·9			
3	,,	Ec. D. Last seen	7 57 7·5	−0 18·5	S. J. P.	Fair, limb boiling, satellite close to limb.
3	,,	Oc. R. First seen	10 12 50·1		,,	Good, limb a little unsteady. Defin. very
		Bisection	14 50·4			good.
		Ext. contact	16 34·6			
8	,,	Oc. D. Ext. contact	15 16 30·1		W. C.	Defin. good, rather unsteady.
		Bisection	18 31·6			
		Last seen	20 35·6			

		Satellite.	Phenomenon.	G. M. T. h m s	In excess of N. A. m s	Observer.	Remarks.
Oct.	9	III.	Oc. D. Ext. contact	7 8 37·7		W. C.	Poor, very hazy, thin clouds.
			Bisection	11 46·9			
			Last seen	14 52·1			
	9	,,	Ec. R. First seen	9 47 0·2	−3 18·8	,,	Good, full brilliancy 4m later. Rather difficult owing to satellite being so near ♃.
	9	II.	Oc. D. Ext. contact	10 39 28·5		,,	Slight haze round ♃. Slightly tremulous but defin. good.
			Bisection	41 27·0			
			Last seen	43 35·4			
	9	I.	Tr. I. Ext. contact	12 32 55·6		,,	Defin. poor, tremulous.
			Bisection	35 12·4			
			Int. contact	36 52·1			
	9	,,	Sh. I. Bisection	12 39 27·7		,,	Tremulous. Sh. faint.
			Int. contact	42 3·2			
	9	II.	Ec. R. First seen	13 24 42·7	−0 21·3	,,	Very good.
	9	I.	Tr. E. Int. contact	14 42 25·9			Defin. good. Tremulous. Sh. too faint to observe in so unsteady a sky.
			Bisection	45 12·7			
			Ext. contact	47 57·9			
	10	,,	Oc. D. Ext. contact	9 41 1·2		S. J. P.	Defin. poor. Satellite projected on limb during occultation. Limb very tremulous
			Bisection	45 24·7			
			Last seen	47 39·7			

	Satellite.	Phenomenon.	G. M. T. h m s	In excess of N. A. m s	Observer.	Remarks.
Oct. 10	I.	Ec. R. First seen	12 4 7·1	+1 25·1	W. C.	Good. Full light only approx.
		Full light	6 19·3			
11	„	Tr. I. Ext. contact	6 55 2·0		S. J. P.	Satellite very bright on disk. Obs. only fair, misty.
		Bisection	57 0·6			
		Int. contact	7 0 43·8			
11	„	Tr. E. Int. contact	9 6 27·1		„	Defin. good, but passing clouds.
		Bisection	10 12·3			
		Ext. contact	13 6·0			Last contact seen through thin clouds. Sh. very small, fairly distinct at times, but clouds too frequent for good obs. of egress of Sh.
19	„	Oc. D. Ext. contact	5 53 23·7		W. C.	Only fair, defin. poor, tremulous.
		Bisection	54 56·7			
		Last seen	56 34·2			
19	„	Ec. R. First seen	8 26 44·3	−0 8·7	„	Fair. Screen used.
		Full light	29 51·9			
23	III.	Oc. D. Ext. contact	13 40 23·8		„	Defin. good, slightly unsteady.
		Bisection	44 40·6			
		Last seen	49 9·0			
23	II.	Oc. D. Ext. contact	15 4 42·7		„	Defin. very poor, tremulous.
		Bisection	7 11·9			
		Last seen	9 2·6			

	Satellite.	Phenomenon.	G. M. T. h m s	In excess of N. A. m s	Observer.	Remarks.
Nov. 1.	II.	Tr. I. Ext. contact	11 56 2·6		W. C.	Defin. not very good.
		Bisection	58 11·8			
		Int. contact	12 0 31·5			
1	I.	Tr. I. Ext. contact	12 9 42·0		„	Defin. poor, unsteady.
		Bisection	10 30·6			
		Int. contact	13 4·8			
1	„	Sh. I. Bisection	12 54 26·6			Fair, defin. improved.
		Full on	56 26·2			
1	II.	Sh. I. Bisection	13 16 21·9		W. C.	Defin. bad. Sh. small and faint.
		Full on	19 8·1			
1	I.	Tr. E. Int. contact	14 21 41·0		„	Defin. fair, tremulous.
		Bisection	23 54·1			
		Ext. contact	26 35·3			
1	II.	Tr. E. Int. contact	14 34 38·8		„	Very unsteady.
		Bisection	36 26·2			
		Ext. contact	38 12·0			
3	„	Oc. D. Ext. contact	6 26 47·6		„	Good.
		Bisection	30 11·1			
		Last seen	33 24·6			

	Satellite.	Phenomenon.	G. M. T. h m s	In excess of N. A. m s	Observer.	Remarks.
Nov. 3	I.	Tr. I. Ext. contact	6 37 58·6		W. C.	Good.
		Bisection	40 40·3			
		Int. contact	43 6·1			
3	III.	Tr. I. Ext. contact	6 47 51·6		,,	Good.
		Bisection	52 34·6			
		Int. contact	57 50·1			
3	I.	Sh. I. Bisection	7 21 38·2		,,	Defin. fair. Sh. exceedingly faint, very difficult observation.
		Full on	23 24·2			
3	,,	Tr. E. Int. contact	8 48 30·9		,,	Defin. good, night improving.
		Bisection	50 38·4			
		Ext. contact	52 50·4			
3	III.	Tr. E. Int. contact	9 11 52·9		,,	Defin. good, sky a little hazy, field white.
		Bisection	18 25·4			
		Ext. contact	23 55·0			
3	I.	Sh. E. Int. contact	9 29 39·5		,,	Shadow faint, but obs. fair.
		Bisection	30 59·5			
		Last seen	32 52·5			
3	III.	Sh. I. Bisection	9 41 29·5		,,	Defin. good. Sh. very small but black.
		Full on	44 41·5			
3	II.	Ec. R. First seen	10 26 40·0	−2 3·0	,,	Very good. Sh. of III. very dark on disk.
		Full light	31 4·6			

	Satellite.	Phenomenon.	G. M. T. h m s	In excess of N. A. m s	Observer.	Remarks
Nov. 3	III.	Sh. E. Int. contact	12 3 26·8		W. C.	Very poor, defin. bad, unsteady.
		Bisection	6 55·8			
		Last seen	10 24·8			
19	II.	Tr. I. Ext. contact	5 47 36·2		„	Very poor. Thin clouds passing.
		Bisection	49 55·7			
		Int. contact	52 23·2			
19	I.	Tr. E. Int. contact	6 48 48·6		„	Good, night much better.
		Bisection	51 2·3			
		Ext. contact	53 25·1			
19	II.	Tr. E. Int. contact	8 26 6·2		„	Rather tremulous, but obs. very good.
		Bisection	29 18·9			
		Ext. contact	31 36·4			
20	I.	Ec. R. First seen	5 6 47·1	−0 19·9	„	
21	II.	Ec. R. First seen	4 56 25·5	−1 38·5	„	Sky hazy, full light 3^{m} later.
21	III.	Oc. R. Bisection	5 51 4·8			Tremulous, but obs. good.
		Ext. contact	54 58·8			
27	I.	Ec. R. First seen	7 2 36·3	−0 19·7	„	Sky rather hazy.
		Full light	4 56·6			

Observations of the Solar Eclipse, 1880, *Dec.* 31, *made at the University Observatory, Strasburg.* By Dr. A. Winnecke.

The chances for getting some observations during the Solar eclipse of Dec. 31 were very poor; nevertheless we got up the Heliometer on the gallery of the building for the great Refractor, whence it was possible to follow the Sun to the horizon, the eclipse ending only a quarter of an hour before his setting.

Heavy clouds obscured the Sun during the first half of the eclipse, but then an opening coming up, we got 8 measures of the distances of the cusps with the Heliometer. After due reduction the chords measured by Dr. Schur and Dr. Hartwig were the following:

Mean Solar Time Strasburg.	Distance of Cusps.	Errors.
h m s	″	″
3 45 25·1	955·74	+1·03
46 34·1	935·59	−1·01
48 20·1	858·28	−1·37
49 58·1	814·43	−0·36
51 7·1	768·81	+2·57
52 3·1	704·80	−0·83
53 41·1	625·41	+1·23
54 39·1	581·59	−1·99

After the last measure clouds again covered the Sun. Using the Ephemeris of the Moon given by the "Connaissance des Temps," and correcting by $-1''\cdot90$ the semidiameter of the Sun and by $-2''\cdot30$ the semidiameter of the Moon,* and putting—

dr = correction to the adopted semidiameter of the Sun,
dR = „ „ „ Moon,
dx = „ excess of Moon's R.A. over Sun's R.A. × cos δ,
dy = „ „ „ Decl. over Sun's Decl.,
i = „ „ point of coincidence of the images formed by the two halves of the object glass,
c = error depending on the direction of moving the screws;

Dr. Schur puts the following equations of condition—

$$0 = -5''\cdot34 + 2\cdot094\,dr + 2\cdot076\,dR - 0\cdot429\,dx - 1\cdot178\,dy + c + i$$
$$0 = -20\cdot71 + 2\cdot174 + 2\cdot158 - 0\cdot482 - 1\cdot859 - c + i$$
$$0 = -3\cdot81 + 2\cdot327 + 2\cdot311 - 0\cdot577 - 2\cdot011 + c - i$$
$$0 = -23\cdot24 + 2\cdot511 + 2\cdot497 - 0\cdot684 - 2\cdot191 - c - i$$
$$0 = -27\cdot88 + 2\cdot680 + 2\cdot656 - 0\cdot775 - 2\cdot355 - c - i$$
$$0 = -9\cdot71 + 2\cdot850 + 2\cdot838 - 0\cdot864 - 2\cdot518 + c - i$$
$$0 = -17\cdot47 + 3\cdot264 + 3\cdot253 - 1\cdot069 - 2\cdot911 + c + i$$
$$0 = -34\cdot90 + 3\cdot627 + 3\cdot618 - 1\cdot241 - 3\cdot253 - c + i$$

* *Monthly Notices*, vol. xxxvi. p. 86.

Neglecting the corrections to the adopted semidiameters (they cannot be exactly made out by these measures), Dr. Schur obtained, by solving the equations by the method of least squares—

Correction to R.A. of the Moon $-13''.42$, with the prob. err. $5''.25$
„ Decl. „ $-3''.56$ „ $1''.61$

The probable error of a single measure is $\pm 1''.38$, very small considering the great zenith distance in which the measures had to be made.

The Heliometer has $0^m.076$ aperture and a focal distance of $1^m.15$; power 135.

1881, *Strasburg, Jan.* 5.

Eclipse of the Sun, Dec. 31, 1880, *observed at the University Observatory, Oxford.* By Prof. C. Pritchard.

The Solar eclipse of Dec. 31 was observed here under exceptionally good atmospheric circumstances, though the small altitude of the Sun interfered with the definition, and, towards the end of the eclipse, the roof of the University Museum obscured some portion of the Sun's disk.

Times of Contact.

	G. M. T.	Telescope.	Observer.	Method.
First contact	1 38 0·3	De La Rue reflector	Mr. Jenkins	E. & E.
„	1 38 2·6	Grubb refractor	Mr. Plummer	Chronograph.
Last contact	3 38 2·0	De La Rue reflector	Mr. Jenkins	E. & E.
„	3 28 3·7	Grubb refractor	Mr. Plummer	Chronograph.

The aperture of the refractor is 12¼ inches, power employed 108
„ reflector is 13 „ „ 180

The distance of the cusps and the position angle of the line joining them were measured with a filar micrometer near the beginning and end of the eclipse. Six measures were secured at the beginning, and three towards the end. At present there has been no opportunity of reducing these measures, and if the results contain any point of interest they will be submitted to the Society.

Oxford,
1881, *Jan.* 13.

Observations of the Solar Eclipse of 1880, *December* 31, *made at the Radcliffe Observatory, Oxford.*
By E. J. Stone, Esq.

Phenomenon.	Instrument.	Clock or Chronometer.	Local Sidereal Time.	Greenwich Mean Solar Time.	Observer.	Reference.
			h m s	h m s		
Beginning of Eclipse	10-foot Equatoreal	Sidereal Chronometer	20 14 22·4	1 37 59·6	Mr. Wickham	(*a*)
Contact of Moon's limb with Penumbra of Sunspot	10-foot Equatoreal	Sidereal Chronometer	20 20 18·9	1 43 55·1	Mr. Wickham	
	42-inch Telescope	Sidereal Chronometer	20 20 19·4	1 43 55·6	Mr. Robinson	
Contact of Moon's limb with Umbra of Sunspot	10-foot Equatoreal	Sidereal Chronometer	20 20 43·7	1 44 19·9	Mr. Wickham	
	42-inch Telescope	Sidereal Chronometer	20 20 50·4	1 44 26·6	Mr. Robinson	
Ending of Eclipse	10-foot Equatoreal	Sidereal Chronometer	22 4 45·7	3 28 4·8	Mr. Wickham	(*b*)
	42-inch Telescope	Sidereal Chronometer	22 4 46·2	3 28 5·3	Mr. Robinson	(*c*)
	Heliometer	Heliometer Clock	22 4 45·2	3 28 4·3	Mr. Bellamy	(*d*)

Remarks.

(*a*) Limb tremulous, but observation good.

(*b*) Sun very low; thin cloud; observation fair.

(*c*) Observation good.

(*d*) Observed through cloud.

Mr. Luff, who was using a small telescope (with power 16, aperture 1 inch) and pocket solar chronometer, noted the following times:—

	h m s	
Beginning of Eclipse	1 38 5·4	Greenwich Mean Solar Time.
Contact with Mean Edge of Sunspot	1 44 11·4	

The Partial Eclipse of the Sun, December 31, 1880.

By J. Rand Capron, Esq.

This, though owing to bad weather not observable in the north, was mostly well seen in the southern parts of England. At Guildown a clouded morning was followed by a clear midday, and, for the time of year, a fairly bright afternoon, so that the phenomenon was visible throughout. The first contact (which, however, from the boiling of the edges of both Sun and Moon would probably have been uncertain) was missed; but a large oval spot lying in front of the Moon's edge, contact with the external edge of the penumbra of this spot was timed at $1^h\,45^m\,35^s$ G.M.T. Subsequently eight photographs of the partially eclipsed Sun's disk were taken with Wratten and Wainwright's dry instantaneous plates, upon the 6-inch Cooke Equatoreal. As to the first seven the image was enlarged with a Barlow lens, no allowance being made for difference in chemical and visual foci. Exposure was made by the sliding of plates of metal with slits of varying width cut in them. In the case of the last plate, the image was the primary one not enlarged. Exposure was purposely prolonged—no diaphragm being used, but the ordinary camera slide was opened and shut by hand as quickly as possible.

This plate shows the Sun's image reversed, white upon a dark grey ground. All the plates show with more or less distinctness a set of small spots in front of the Moon's limb, which is fairly well defined and serrated. The Moon's disk in contact with the Sun presents a bright line of light round it which I attribute to photographic irradiation.

I have obtained a similar result by photographing a black velvet disk suspended against a bright sky. The Moon's body was not observed external to the Sun's disk, but much mist was present even before first contact. No marked darkening of daylight was noticed as the result of the partial obscuration of the Sun, which set uneclipsed in a fine red glow. The times of exposure of the plates 3 to 8 were as under. The plate 8 shows the eclipse at very nearly total phase.

Times of Exposure of Plates.

No. 1	Not timed.			No. 5	h	m	s
2					2	18	0
	h	m	s	6	2	25	0
3	2	7	30	7	2	28	30
4	2	14	0	8	2	30	0

Guildown, Guildford,
1881, *January* 10.

On some Difficulties connected with the Determination of the Diameter of Mars. By E. J. Stone, Esq.

Our determinations of the diameters of the planets are complicated by the existence of systematic personal errors in the observers and, to some extent, by constant errors depending upon the instrument used. But the determinations of the diameter of *Mars* which have been made from time to time, have differed so much amongst themselves that there are grounds for suspecting the existence of some source of systematic error which has been overlooked.

Thus, in the *Monthly Notices* for November, 1880, Mr. Downing has inferred, from a discussion of all the measures of the vertical diameter of *Mars* which have been made with the Greenwich Transit Circle, that Le Verrier's value of the angular diameter of *Mars*, at the Sun's mean distance, 11″·10, requires to be diminished to 9″·70. The results deduced from two extensive series of observations, with many changes amongst the observers, and with different adopted values of the diameter, are closely accordant; and the result has been correctly deduced from the observations on the assumptions made. But Le Verrier's result equally represents the observations on the assumption which he made.

The observations at Greenwich are made by a large number of persons; and, although some errors, of an unsystematic character, do thus creep into the work, and the discussion of the results is greatly complicated, the facilities afforded for the examination of questions connected with personal errors are very great.

I have collected, from the Greenwich volumes, the measures of the vertical diameters of *Mars* made 1851 to 1878, and assuming that the true value of the angular diameter $=\text{Tab.}(1+y)$, the personal errors of the observers are given on two suppositions.

Table I.

Personal Equations.

With Le Verrier's Diameter 11″·10 under the heading V.
With Downing's Diameter 9″·697 under the heading D.

Period 1851–1865.

Observer's Name.	V.	D.	Factor of *y*.	Number of Observations.
	″	″	″	
Breen, J.	−0·29	+1·35	−10·40	1
Breen, H.	+0·23	+2·28	−13·00	2
Bowden	+0·42	+2·53	−13·40	1
Carpenter, J.	−1·34	+0·74	−13·18	15
Criswick	−0·83	+1·54	−15·01	31

Observer's Name.	V.	D.	Factor of y.	Number of Observations.
Christy, W. J.	$-1''\cdot46$	$+0''\cdot14$	$-10''\cdot01$	2
Chappell	$+1\cdot23$	$+3\cdot87$	$-16\cdot71$	11
Davis	$-1\cdot84$	$+1\cdot35$	$-20\cdot20$	4
Dolman	$-1\cdot25$	$+2\cdot14$	$-21\cdot45$	2
Dunkin	$-0\cdot66$	$+1\cdot39$	$-13\cdot01$	40
Eaton	$+0\cdot85$	$+3\cdot92$	$-19\cdot37$	6
Ellis, W.	$+1\cdot09$	$+3\cdot39$	$-14\cdot60$	54
Henry	$+0\cdot28$	$+2\cdot17$	$-11\cdot88$	15
Henderson	$+0\cdot30$	$+2\cdot06$	$-11\cdot10$	11
Kerschner	$-0\cdot86$	$+1\cdot89$	$-17\cdot43$	12
Lynn	$+1\cdot40$	$+3\cdot60$	$-13\cdot92$	12
Main	$+0\cdot80$	$+2\cdot88$	$-13\cdot20$	1
Newcomb	$+5\cdot14$	$+7\cdot03$	$-12\cdot00$	1
Nash	$-0\cdot89$	$+1\cdot50$	$-15\cdot33$	3
Roberts	$-1\cdot11$	$+1\cdot48$	$-16\cdot38$	8
Rogerson	$+1\cdot19$	$+3\cdot15$	$-12\cdot36$	5
Stone	$+1\cdot73$	$+4\cdot08$	$-14\cdot85$	6
Taylor, H.	$+0\cdot02$	$+1\cdot90$	$-11\cdot85$	4
Taylor, F.	$+0\cdot31$	$+2\cdot39$	$-13\cdot20$	1
Talmage	$+3\cdot76$	$+4\cdot55$	$-5\cdot00$	1
Wakelin	$-1\cdot62$	$+0\cdot81$	$-15\cdot40$	4
Weighted Means	$+0\cdot06$	$+2\cdot32$	$-14\cdot32$	253 = Sum

These coefficients of y require to be increased in the proportion of $\frac{11\cdot10}{8\cdot87}$ or 1·25 to be strictly comparable with those of Table II. This correction has been allowed for in the work.

TABLE II.

Personal Equations.

With Le Verrier's Diameter $11''\cdot10$ under the heading V.
With Downing's Diameter $9''\cdot697$ under the heading D.
Period 1866–1878.

Observer's Name.	V.	D.	Factor of y.	Number of Observations.
Baker	$+2''\cdot01$	$+3''\cdot73$	$-13''\cdot65$	4
Bromley	$-1\cdot26$	$+2\cdot19$	$-27\cdot60$	4
Christie, W. H. M.	$-1\cdot74$	$-0\cdot04$	$-13\cdot52$	5
Criswick	$-0\cdot37$	$+1\cdot78$	$-15\cdot77$	40
Carpenter, J.	$-0\cdot79$	$+0\cdot95$	$-13\cdot89$	20
Carpenter, H.	$+0\cdot74$	$+2\cdot67$	$-15\cdot25$	8

Observer's Name.	V.	D.	Factor of y.	Number of Observations.
	″	″	″	
Dennison	−0·94	+1·25	−17·43	3
Dunkin	−0·70	+1·09	−14·17	6
Downing	−0·14	+2·13	−18·04	26
Ellis	+1·12	+3·01	−15·00	23
Graham	+2·62	+5·03	−19·14	7
Harding	+0·15	+2·62	−19·60	1
Jenkins	+0·71	+2·96	−17·94	7
Keating	+0·18	+2·84	−21·10	2
Kerschner	+1·83	+3·68	−14·65	4
Laird	+1·63	+3·56	−15·28	5
Lynn	+2·34	+4·19	−14·73	18
Pett	−1·14	+1·27	−19·10	3
Pead	−2·03	+1·23	−25·90	3
Plummer	+1·15	+2·85	−13·50	2
Potts	+1·11	+3·16	−16·28	6
Power	+0·12	+2·39	−18·18	6
Pulley	−0·49	+1·44	−15·26	7
Robinson	+0·04	+2·56	−20·20	7
Thackeray	+0·81	+3·35	−20·13	21
Wickham	+0·06	+2·73	−21·15	2
Wright	−0·02	+1·92	−15·40	1
Weighted Means	+0·33	+2·47	−16·74	241 = Sum

It will be seen that the measures of different observers may differ by nearly 3″ in cases where the number of observations would render any mere errors of observation small, and where the factors of the adopted diameter are too nearly alike to render any adjustment of these differences by a change of the adopted diameter possible. A comparison of the results of the two series will also show that these personal differences remain, for the principal observers, almost unchanged, and that the methods of observing contacts adopted by experienced observers sensibly and systematically differ, or that real personal equations to a very sensible amount do, as a matter of fact, exist in the measures of the diameter of *Mars*.

The results given in these tables appear to me of some practical importance: they afford a very necessary caution against the acceptance of any measured diameters as definitive when all the measures have been made by the same person, unless a sufficient range exists amongst the measured diameters to allow of an accurate separation of the constant and variable parts of the observed diameter.

If we adopt Le Verrier's value, the mean error of all the observed diameters in Table (1) are destroyed, and the mean error in Table (2) is very small. It is clear, therefore, that Le Verrier has assumed that, in the mean of a large number of measures made by many different observers, the personal errors will mutually destroy each other's effects. The legitimacy of this assumption would appear to be confirmed by the smallness of the mean residual errors in both series of the Greenwich observations, although very considerable changes in the observing staff have taken place. But, of course, Le Verrier's assumption would be untrue, and his result vitiated to a corresponding extent, if there be any systematic source of error which affects the measured diameters of *all the observers* in the same way. Such a source of constant error is presented by telescopic irradiation.

The formula adopted by Mr. Downing is one which has previously been frequently adopted in similar discussions, and distinctly contemplates the existence of *constant* errors in the measures. Mr. Downing assumes that

$$x + y\,(\text{Tab. Diameter}) = \text{Obs. Diameter} - \text{Tab. Diameter},$$

and attempts to determine both x and y from the equations of condition. But, unless the observations are all made by the same person, or corrections are applied for personal errors, an assumption is virtually made that the measures of large and small diameters are equally distributed amongst the different observers. If this condition is satisfied, and the variations between the factors of y are sufficiently large to allow of an accurate separation of x and y, then the diameter thus determined has certainly greater claims to our acceptance than that found by putting $x=0$, which is really the form adopted by Le Verrier. I have separately discussed the observations made by the principal observers, who have observed diameters over a sufficient range; the results confirm the value of y obtained by Mr. Downing, and prove, as might be expected from the large number of observations employed, that the distribution of the measures has been sufficiently uniform to avoid any serious effects on the determination of y. But an inspection of Tables (1) and (2) shows that it is impossible to accept Mr. Downing's value of the diameter of *Mars* without accepting as a fact that the error of nearly 500 measures of the diameter of *Mars* made with an 8-inch telescope by 47 observers is more than $2''$.

It appears to me very difficult to accept such a conclusion. The irradiation constant for the planet *Mars*, with such a telescope, can hardly amount to one-fifth of the quantity under consideration; and, although the mean personal error of the observers is included in the quantity, it is difficult to conceive that the mean personal error of so many observers can amount to nearly as large a quantity as the whole range, of personal error, which is well established amongst the large number of observers whose measures are compared in the present

paper. It appears to me, therefore, that there must be some systematic error which vitiates the result by rendering untrue the assumption that all deviations of the measured diameters from the formula

$$x + y\,(\text{Tab. Diameter}) = \text{Obs. Diam.} - \text{Tab. Diam.}$$

arise from mere chance errors of observation. If we represent the errors geometrically by points, whose abscissæ are the tabular diameters and ordinates the excess of observed diameters over the tabular diameters, we ought, on the suppositions which underlie our investigations, to be able to draw a straight line amongst these points, so that there should be no unequal preponderance of points on either side of the line throughout its length. I have traced the curves for several of the observers, and find that such is not the case. To test the point I have proceeded as follows:—

(1) All the values of excess of observed over tabular diameters have been reduced to an uniform standard with the assumed value of 11″·10 at the mean distance.
(2) Corrections for personal equations have been applied from the Tables (1) and (2).
(3) No observations have been employed, unless the same observer has made observations when the diameter was very small, near the mean, and large. This avoids any errors creeping into the work from imperfect correction for personal error.

The measures were then divided into groups with tabular diameters:—

5″–7″; 7″–9″; 9″–12″; 12″–15″; 15″–17″;
17″–19″; 19″–22″; 22″–25″; above 25″.

The following table gives the results:—

Mean Factor.	No. of Observations.	Assumed Diameter at Earth's Mean Distance = 9″·71	= 10″·73	= 11″·10
7″·48	5	+2″·94	+2″·24	+2″·00
9·11	25	+2·39	+1·54	+1·24
12·21	49	+2·15	+1·01	+0·61
15·32	68	+2·05	+0·62	+0·12
17·80	80	+2·25	+0·60	+0·01
20·85	28	+1·76	−0·18	−0·87
23·21	35	+2·36	+0·21	−0·56
24·43	18	+2·15	−0·11	−0·92
27·33	11	+2·50	−0·04	−0·94

If these points be laid down to scale, the break of continuity in passing from the smaller measures is distinctly seen. It will be found that the straight line represented by Mr. Downing's solu-

tion is the best straight line which can be drawn through these points, but that it leaves all the intermediate points on one side of the line. Le Verrier's solution destroys the mean error, but leaves the extreme measures unrepresented. The other value 10″·73 destroys the mean error for the larger measures. The arbitrary break about 16″ is seen with all the different adopted values of the diameter at the Sun's mean distance, and shows, I venture to think, that either the same thing is not attempted to be measured when the diameter is about 8″ as when the diameter is over 16″, or that some very serious error exists in the calculation of the distance of *Mars*. The last supposition is, I believe, inadmissible to the requisite amount, and we appear, therefore, driven to suppose that the true limb is not seen when *Mars* is very distant from us, but that the measures have reference to the visible boundary of the atmosphere of *Mars*, which is itself not of a very permanent character at varying distances. After 16″ the measures of the diameter appear more consistent, and from these alone we should obtain

$$10''.73 - 0.04 \times i$$

for the value of the diameter at the mean distance: i is the irradiation constant, which can scarcely exceed 0″·5 for observations of *Mars* with an 8-inch telescope, and may be less. But this value only represents the larger measures, and is certainly not a satisfactory determination of the diameter.

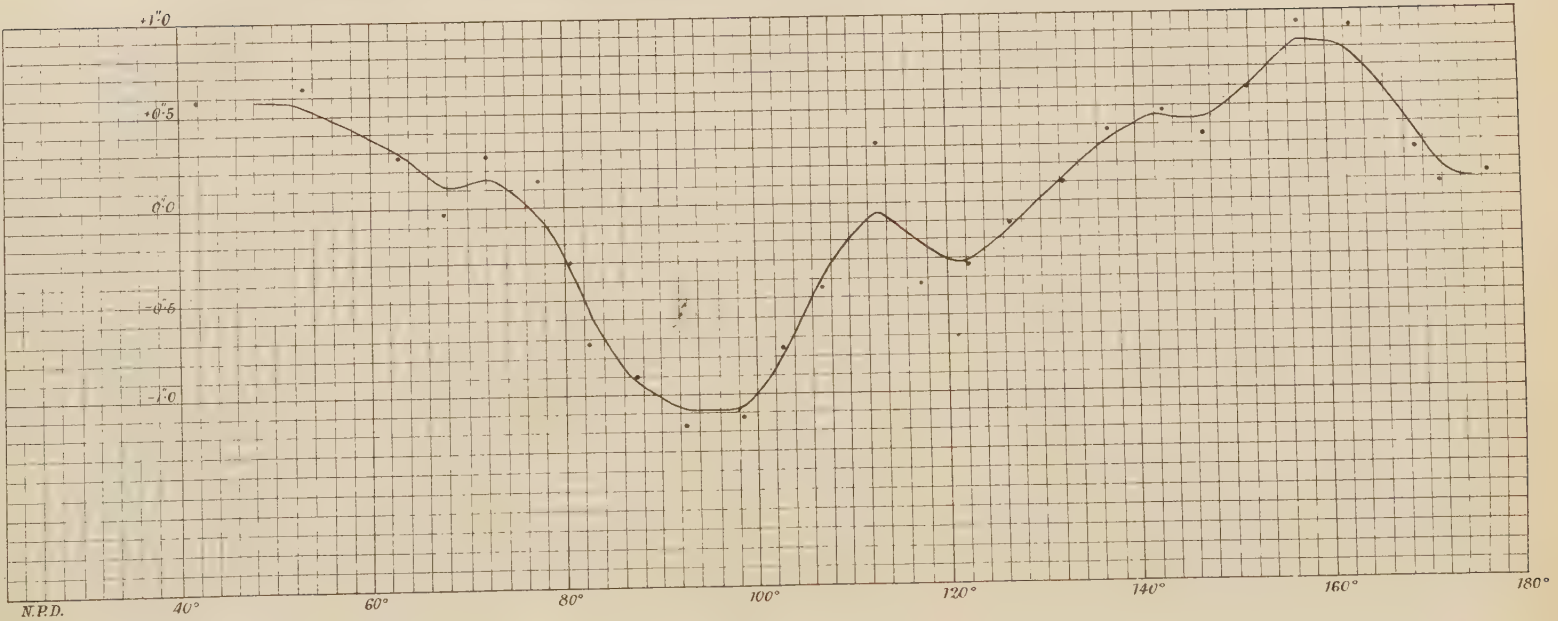
It appears essential that observations of single limbs of *Mars* should be reduced with diameters determined by the same observer, and, if possible, at distances of *Mars* not very different from the distance at the time of observation. I have not examined the Right Ascension observations of diameter, but they are hardly sufficient in number to throw much light upon the question of the supposed sensible variation in the *effective* diameter at different distances.

On the North Polar Distances of the Cape Catalogue for 1840.

By A. M. W. Downing, Esq.

The Cape Catalogue for 1840, published by Mr. Stone in 1878, is deduced from observations made under the direction of the late Sir T. Maclear during the years 1832 to 1840 with the six-feet mural Circles by Jones. The old instrument, erected by Fallows 1828–1829, was used until July 1839, when it was replaced by the new mural Circle, which was mounted in the same position. The zenith points of the mural Circles were determined by direct and reflexion observations of stars at the meridian passage, and it has been usual to adopt results deduced from observations made at intervals extending from a week to a month, according to the stability of the instrument. Corrections for discordance of direct and reflexion observations have been determined by a graphical process and applied.

COMPARISON OF THE NORTH POLAR DISTANCES OF THE CAPE CATALOGUES, FOR 1860 AND FOR 1840.



Shottiswoode & Co. Lith. London.

The refractions used in the reductions were those of Bessel's *Tabulæ Regiomontanæ*.

The division errors of the mural Circles do not appear to have been determined. The flexure was considered insensible.

The places given in the Catalogue have been corrected for proper motion in only a few instances; in the present investigation a correction for proper motion has been applied to the Catalogue place whenever it became significant.

The particulars given above have been extracted from the Introduction to the Catalogue written by Mr. Stone; as the work is not yet distributed to the astronomical public, it appeared desirable to reproduce them in this place.

The North Polar Distances of this Catalogue have first been compared with those of the Cape Catalogue for 1860. There are 398 stars available for the comparison, after rejecting those whose places in either Catalogue depend on a single observation. The N.P.D.'s of these, as given in the 1860 Catalogue, were brought back to the epoch 1840 by applying the precessions, secular variations, and generally the proper motions of the 1860 Catalogue. In some instances these proper motions appeared to be erroneous, in which cases what was considered a more correct value was used instead of that given in the Catalogue. The differences between the N.P.D.'s of the 1860 Catalogue thus brought back and those of the 1840 Catalogue were then taken, and having been arranged in order of N.P.D., were combined in groups extending over 5° of N.P.D.; the means of the differences for each group being considered as the difference between the Catalogues for the mean N.P.D. of the group. These mean differences were then laid down as points on cross-ruled paper, and a curve (*see* diagram) drawn through them, which may be taken to represent the systematic differences of the N.P.D.'s of the Catalogues.

The differences as computed and as read off from the curve are as follows:—

N.P.D.	Number of Stars.	1860−1840 Means.	1860−1840 Curve.
° ′		″	″
41 57	3	+ ·57	+ ·57
53 7	3	+ ·66	+ ·55
62 16	12	+ ·28	+ ·30
67 43	22	− ·04	+ ·12
72 3	8	+ ·29	+ ·17
77 49	6	+ ·15	− ·04
82 34	14	− ·72	− ·55
87 3	8	− ·90	− ·92
92 11	4	−1·16	−1·09
98 4	13	−1·13	−1·06
103 0	8	− ·78	− ·79
107 29	15	− ·43	− ·34
112 50	18	+ ·32	− ·06

N.P.D.	Number of Stars.	1860−1840 Means.	1860−1840 Curve.
° ′		″	″
117 36	40	− ·42	− ·22
122 33	34	− ·35	− ·31
127 16	27	− ·11	− ·12
132 21	28	+ ·11	+ ·13
137 20	24	+ ·39	+ ·35
142 50	19	+ ·48	+ ·43
147 29	22	+ ·36	+ ·45
152 19	19	+ ·60	+ ·63
157 16	21	+ ·96	+ ·86
162 55	3	+ ·91	+ ·77
168 30	9	+ ·27	+ ·38
171 56	12	+ ·07	+ ·14
176 58	6	+ ·14	+ ·14

The following table gives the differences for every 4° of N.P.D. as read off from the curve, and also the comparisons with the Greenwich Catalogues for 1860 and for 1840, which are deduced from the table given at p. 172 of Mr. Christie's paper in vol. xlv. of the *Memoirs*.

N.P.D.	Cape 1860 *minus* Cape 1840.	Greenwich 1860 *minus* Cape 1840.	Greenwich 1840 *minus* Cape 1840.
°	″	″	″
48	+ ·57	+ 1·16	+ 1·03
52	+ ·55	+ 1·12	+ ·88
56	+ ·48	+ 1·02	+ ·64
60	+ ·38	+ ·89	+ ·49
64	+ ·25	+ ·73	+ ·35
68	+ ·13	+ ·59	+ ·27
72	+ ·17	+ ·62	+ ·43
76	+ ·01	+ ·42	+ ·42
80	− ·28	+ ·07	+ ·20
84	− ·69	− ·38	− ·09
88	− ·96	− ·62	− ·24
92	− 1·08	− ·71	− ·27
96	− 1·08	− ·67	− ·17
100	− ·99	− ·58	− ·09
104	− ·70	− ·37	+ ·05
108	− ·30	·00	+ ·23
112	− ·07	+ ·19	+ ·12
116	− ·16	+ ·04	+ ·15
120	− ·29	− ·16	+ ·41
122	− ·32	− ·19	+ 2·08
124	− ·27	− ·09	+ 3·93
128	− ·09		
132	+ ·11		
136	+ ·29		
140	+ ·41		
144	+ ·44		
148	+ ·47		
152	+ ·60		
156	+ ·81		
160	+ ·82		
164	+ ·70		
168	+ ·42		
172	+ ·13		

Comparisons with the other Greenwich Catalogues and with the Melbourne Catalogue can be found at once from the table in Mr. Christie's paper referred to above.

It should be remarked that the Cape and Greenwich Catalogues for 1840 give quite independent results to the Catalogues for the epoch 1860, as different instruments were in use at both Observatories at these dates.

A direct comparison has also been made between the Cape and Greenwich Catalogues for 1840, with results substantially agreeing with those given above. The number of stars used is 281, and proceeding generally as before the computation gives:—

Z.D. at Cape.	N.P.D.	Number of Stars.	Greenwich − Cape 1840	
			Means.	Smoothed Means.
	° ′		″	″
87	37 6	5	+5·59	+5·59
83¼	40 49	6	+2·86	+2·86
77½	46 26	18	+1·70	+1·70
73¼	50 45	6	+0·50	+ ·86
65¼	58 44	3	+ ·76	+ ·63
61½	62 35	13	+ ·49	+ ·50
56¼	67 49	26	+ ·23	+ ·35
51⅓	72 40	14	+ ·43	+ ·42
46¾	77 11	7	+ ·57	+ ·45
41½	82 24	17	+ ·23	+ ·16
Z.D. at Greenwich.				
48½	87 1	11	− ·40	− ·16
53½	92 4	6	+ ·01	− ·13
60	98 21	17	− ·12	+ ·02
64¼	102 47	13	+ ·29	+ ·15
69	107 22	24	+ ·12	+ ·28
74¾	113 15	24	+ ·55	+ ·48
78⅓	117 49	36	+ ·69	+ ·69
84	122 30	13	+1·56	+1·56
86¾	125 14	22	+2·81	+2·81

The smoothed means have been obtained by taking the means of every consecutive pair, omitting the two extreme values at each end of the series, and the means of these again; being in fact the same process as has been adopted in drawing the curve for comparison of the two Cape Catalogues.

The most noticeable feature in the comparison of the Cape Catalogues is the large difference in the neighbourhood of N.P.D. 95°; this is not to be attributed to large actual error in either of the Catalogues, as the comparison with Greenwich seems to show that it is about equally divided between them.

With regard to the question of astronomical refraction, the evidence afforded by this investigation, as far as it goes, seems to confirm the results deduced from the discussion of the N.P.D.'s of the Cape 1860 and Melbourne 1870 Catalogues—viz., that down to about 80° Z.D. the Greenwich observations are satisfactorily represented by the refractions of the *Tabulæ Regiomontanæ*, and that below that limit some such corrections as those proposed by Mr. Main are required. Thus, in the table above, the correction at 84° Z.D. is about $-1''$, and that at $86\frac{3}{4}°$ is about $-2''.5$, and the discordances are then reduced to $+.56$ and $+.31$ respectively. At the same time it will be remarked that the Cape observations near the north horizon appear to require refractions diminished in a larger proportion, and corrections commencing at a less zenith distance than is the case at Greenwich. On this point, however, further investigation is desirable, the materials for which are now, we may hope, rapidly accumulating.

The comparison of N.P.D.'s of principal "Nautical Almanac" stars in the Greenwich and Cape Catalogues for 1840, given in the Introduction to the latter, has been utilised, for the stars situated between N.P.D. 60° and 118°, for the purpose of ascertaining whether there is any outstanding correction depending on the meteorological elements. For this purpose these stars have been combined in groups extending over six hours of R.A., with the result :—

R.A.	0^h–6^h	6^h–12^h	12^h–18^h	18^h–24^h
Greenwich – Cape	$-''.02$	$+''.08$	$+''.53$	$+''.22$.

If $+''.10$ be taken as the systematic difference between the Catalogues for the limits of N.P.D. under consideration, we have—

R.A.	0^h–6^h	6^h–12^h	12^h–18^h	18^h–24^h
Greenwich – Cape	$-''.12$	$-''.02$	$+''.43$	$+''.12$.

The similarity of this result to that given by Mr. Stone as deduced from his own observations (*M. N.* vol. xl. p. 70) is very striking, and remarking the large relative discordance in the third group—that is, for observations made during May, June and July—tends to confirm his opinion that the complete reversion of the seasons at the northern and southern Observatories is not quite accurately allowed for in the refraction tables.

On the Apparent Conjunctions of the Satellites of Uranus with each other, 1881. By A. Marth, Esq.

When the apparent orbits of the satellites are flat ellipses, as they are at present, the satellites will not rarely pass one another at a short distance. Usually they will come to conjunction by encountering one another while moving in opposite directions

on different sides of the great axes of their orbits, but sometimes they will overtake one another while moving in the same direction. When two satellites are at or near conjunction, measurements of their position-angles in reference to the planet's centre cannot well be made with confidence, as the observer's judgment will not be unaffected, but the observation of their conjunction in distance, or the fixing of the time when they are at exactly the same distance from the planet, can be made with nicety, even without micrometer, and such observations will furnish very valuable tests for the theories of the satellites. I have therefore considered it worth while to make the present addition to the Ephemeris published on pages 67–72. During the interval of time over which the Ephemeris extends, some 160 conjunctions of satellites occur, the circumstances of which had to be examined. As I have not yet learnt how close to the planet the satellites can be seen in some of the most powerful telescopes, the limit of about 10″, which I have adopted, may perhaps be too exclusive. Disregarding all those conjunctions which take place within this limit of distance from the planet's centre, and those which on account of daylight and other hindrances offer no fair prospect of being observable in Europe or America, I give for the rest, in the following list, the position-angles and distances of the satellites for the nearest preceding and following even hour G.M.T., so that the circumstances of each conjunction may be seen at a glance:—

			Ariel.		*Umbriel.*		*Titania.*		*Oberon.*	
	1881.	G.M.T. h	Pos. °	Dist. ″	Pos. °	Dist. ″	Pos. °	Dist. ″	Pos. °	Dist. ″
Febr.	18	8	—		200·1	11·4	187·8	16·0	—	
		10	—		198·8	13·6	186·6	14·2	—	
		12	—		197·9	15·5	185·2	12·3	—	
	20	12	—		19·9	13·9	—		6·7	19·4
		14	—		17·9	15·8	—		5·8	17·8
		16	—		17·1	17·4	—		4·7	16·1
	27	10	197·1	12·8	—		—		183·6	15·8
		12	196·0	14·2	—		—		182·1	14·1
		14	195·2	15·1	—		—		180·2	12·4
March	3	8	15·5	14·9	—		7·5	17·7	—	
		10	14·6	15·3	—		6·4	15·8	—	
		12	13·8	15·1	—		4·8	13·7	—	
	4	16·0	194·7	15·3	—		203·1	15·2	—	
		16·6	194·5	15·3	—		202·7	15·7	—	
	7	10	192·2	13·3	195·5	20·7	189·6	22·9	210·6	12·1
		12	190·7	11·5	195·0	21·2	188·9	21·3	208·4	13·8
		14	188·8	9·2	194·4	21·3	188·2	19·6	206·7	15·5
		16	—		193·9	21·1	187·3	17·9	205·4	17·1

			Ariel.		*Umbriel.*		*Titania.*		*Oberon.*	
		G.M.T.	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.
	1881.	h	°	″	°	″	°	″	°	″
March		18	—		193·3	20·6	186·2	16·1	204·2	18·8
		20	—		192·7	19·7	184·8	14·2	203·3	20·4
	18	18	—		—		16·3	32·3	10·2	34·8
		20	—		—		16·0	33·0	9·7	33·3
	26	8	11·2	13·0	—		27·4	12·9	—	
		10	9·7	11·2	—		25·4	14·8	—	
		12	—		12·0	19·3	23·9	16·6	—	
		14	—		11·2	18·0	22·7	18·4	—	
	27	14	191·5	13·2	203·8	10·1	—		—	
		16	189·9	11·4	201·5	12·3	—		—	
		18	187·5	9·1	199·8	14·4	—		—	
	28	14	—		—		12·0	31·7	20·4	30·2
		16	—		—		11·7	30·8	19·9	31·5
April	2	8	20·4	10·0	—		—		352·1	11·0
		10	18·3	11·6	—		—		347·9	9·4
	7	8	22·0	8·8	—		1·4	13.6	—	
		10	19·4	11·1	—		359·0	11·8	—	
		12	17·6	13·0	—		355·6	9·9	—	
	8	14	—		—		204·1	17·0	183·0	20·4
		16	—		—		202·8	18·8	181·8	18·8
	14	10	—		—		14·2	34·4	9·5	35·2
		12	—		—		13·9	34·3	9·1	34·0
	15	10	—		17·3	14·5	9·2	25·8	1·1	18·4
		12	—		16·6	16·3	8·6	24·4	359·6	16·8
		14	—		16·1	17·8	7·9	22·9	357·8	15·2
		16	—		15·6	19·1	7·1	21·4	355·5	13·6
		18	—		15·2	20·0	6·1	19·7	352·7	12·0
	18	16	202·8	8·4	185·8	11·8	—		—	
		18	199·9	10·8	182·9	9·6	—		—	

On January 23 and 24 *Ariel* and *Titania*, moving in the same direction, will remain near one another; on January 27 and 28, *Umbriel* and *Titania*; on March 3 and 4, *Ariel* and *Titania*; on April 9, *Ariel* and *Oberon*; on April 25, *Umbriel* and *Titania*; on May 19 and 20, *Ariel* and *Oberon*.

I need not hint that when the two faint satellites or the two brighter satellites are at equal distances from the planet and near each other, the conditions are most favourable for comparing their brightness.

In March next a century will have elapsed since the dis-

covery of *Uranus* by Herschel. What is the present state of the group of stars which Herschel represents on Tab. xxv. p. 500 of vol. lxxi. of the "Philosophical Transactions"? From March 13 to April 19, 1781, Herschel determined the places of *Uranus* by measuring its distances and position-angles in reference to six stars, $a, \beta, \gamma, \delta, \epsilon, \zeta$. Of these stars of comparison, only a and ϵ, so far as I know, have been observed with meridian instruments—a, 1782, February 5 and 7, at Greenwich, and about 1840 by Rümker; and ϵ, by Rümker, in whose catalogue the places of three other stars of Herschel's figure are also found. The approximate positions of these stars for 1855, as given in the "Bonner Durchmusterung," and the more accurate positions for 1881·0, as deduced from Rümker's places for 1836, with the Pulkova precession constants, are the following:—

		1855.			1881.		
		α	δ		α	δ	
	m	h m s	° ′		h m s	° ′ ″	
a	9·0	5 40 30·4	+23 34·0		5 42 6·06	+23 35 6·7	
	9·0	40 52·8	38·9				
β	9·4	41 11·9	37·0				
γ	9·5	41 37·2	36·5				
δ wanting.							
η	8·7	41 43·3	25·9		5 43 17·82	23 26 6·2	
	9·1	42 20 8	49·6		5 43 55·71	23 50 15·7	
θ	8·8	42 26·3	52·8		5 44 0·99	23 53 30 8	
ϵ	8·8	44 6·1	34·0		5 45 40·68	+23 34 46·8	
ζ	9·4	5 44 17·3	+23 38·7				

Will not observers determine the places of the stars visible between the limits $5^h\ 42^m$ and $5^h\ 46^m$ in Right Ascension, and $23°\ 25'$ and $23°\ 55'$ in Declination, and make a map of the region? Herschel's sketch was made with a 7-inch reflector, so that no more powerful telescope would be required.

Observations of Comet e 1880 (*Swift*), *made at Dun Echt Observatory with the Filar Micrometer of the* 15·06 *inch Refractor.*

(*Communicated by the Earl of Crawford.*)

	Date.	Dun Echt Mean Time.	$\delta - *$ $\Delta\alpha$	$\Delta\delta$	$\delta\alpha$	$\delta\delta$	Observer.
		h m s	m s	′ ″	h m s	° ′ ″	
a	1880, Nov. 7	15 30 19	+ 0 6·87	− 3 5·9	22 45 53·83	+42 33 43·3	J. G. L.
b	9	6 59 37	+ 4 31·27		22 57 58·10		,,
	9	7 27 1		− 8 19·4		+44 30 41·4	,,
c	10	6 10 29	+ 0 37·04	− 8 24·7	23 5 47·14	+45 35 54·2	R. C.
d	12	6 59 14	+ 0 4·89	+ 7 41·1	23 24 2·89	+47 51 33·2	,,
e	19	9 32 42	+ 0 10·24		0 48 52·03		J. G. L.
		10 14 6		+ 6 39·0		+53 50 35·3	,,
f		11 41 17	− 0 36·45		0 50 3·71		,,
		11 56 37		+ 7 10·0		+53 52 38·9	,,
g	20	5 58 26	+ 0 29·62	− 7 16·4	1 0 53·66	+54 13 12·1	,,
h	24	6 42 43	+ 2 11·58		1 59 15·03		,,
		7 6 59		−10 45·1		+54 52 42·0	,,
i	24	6 43 25	− 8 2·23				,,
		7 6 59		− 9 57·8			,,

	Date.	Dun Echt Mean Time.	δ − ✶ Δα	Δδ	αδ	δδ	Observer.
		h m s	m s	′ ″	h m s	° ′ ″	
k	Nov. 25	7 11 40	+ 0 43·29	− 8 3·8	2 13 31·85	+54 43 49·2	J. G. L.
l	25	7 38 28	+ 1 7·07	− 3 26·7	2 13 47·05	+54 43 33·7	,,
m	27	7 54 36	− 1 16·92	− 4 17·4	2 41 3·88	+54 6 2·3	,,
n	29	8 32 2		+ 2 35·0		+53 5 7·7	,,
		8 33 34	+10 5·06		3 6 19·23		,,
o	Dec. 1	13 0 37		− 0 0·5		+51 39 41·4	,,
		13 26 8	+ 7 2·04		3 30 46·35		,,
p	6	12 55 10	+ 0 0·22	− 1 14·4			,,
q	8	8 25 48	− 2 3·78		4 27 25·36		,,
		8 51 2		+ 9 51·8		+46 9 25·2	,,
r	9	12 48 10		− 5 48·3		+45 10 39·8	,,
		13 11 8	− 4 29·36		4 34 48·98		,,
s	1881, Jan. 4	12 14 11	+ 0 3·16	−3 37·4	5 50 45·39	+29 20 9·4	,,
t	6	13 38 36	+ 0 8·45	−0 46·4	5 53 42·95	+28 35 31·4	,,

Adopted Mean Places of Comparison Stars for 1880.

	α	Red.	δ	Red.	Authorities.
	h m s	s	° ′ ″	″	
a	22 45 43·12	+3·84	+42 36 10·5	+38·7	Lalande (Asten) 44726–7.
b	22 53 22·93	+3·90	+44 38 21·5	+39·3	½ (Bonn Cat. + 44°4293 + B.W. xxii. 1199).
c	23 5 6·06	+4·04	+45 43 39·3	+39·6	Arg. Oe. 25230.
d	23 23 53·71	+4·29	+47 43 12·2	+39·9	Arg. Oe. 25644.
e	0 48 36·08	+5·71	+53 43 18·2	+38·1	Rümker, N. F. 393.
f	0 50 34·42	+5·74	+53 44 50·9	+38 0	Bonn Cat. + 53°184.
g					μ Cassiop., "Conn. des Temps."
h	1 56 56·61	+6 84	+55 2 54·2	+32·9	Lalande (Asten.) 3757-8.
i	2 7 13·4	+6·98	+55 1·9	+31·8	D. M. + 54°500.
k	2 12 41·52	+7·04	+54 51 21·7	+31·3	Arg. Oe. 2642.
l	2 12 32·95	+7·03	+54 46 29·1	+31·3	Arg. Oe. 2638.
m	2 42 13·45	+7·35	+54 9 51·8	+27·9	Arg. Oe. 3201.
n					γ Persei, Auwers, "Fundamental Catalogue."
o	3 23 36·78	+7·53	+51 39 19·2	+22·7	Bonn Cat. + 51°744.
p	4 14 40·4	+7·54	+47 43·1	+15·3	D. M. + 47°985.
q	4 29 21·67	+7·47	+45 59 20·2	+13·2	Arg. Oe. 4980.
r	4 39 10·89	+7·45	+45 16 16·5	+11·6	Arg. Oe. 5159.
			Adopted Mean Places of Comparison Stars for 1881.		
s	5 50 39·33	+2·90	+29 23 46·5	+0·3	Observed with the Dun Echt Transit Circle.
t	5 53 31·60	+2·90	+28 36 17·8	+0·0	,, ,,

Remarks.

Nov. 7. At $11^h\ 10^m$ Dun Echt M. T., this comet was independently found by Mr. J. G. Lohse. Owing to bad weather no exact observation could be secured until $15^h\ 30^m$. Very soon after the last measure, the comet disappeared in the rays of the 5·0 mag. star D. M. +42°4521 over which it must have subsequently passed; in the meantime the approaching daylight prevented further observations.

Nov. 10. To the original observation of the comparison star in Bonn Obs. I. p. 94, there is a note that "die Fäden stimmen nicht; sie geben $52^s{\cdot}78$ und $53^s{\cdot}34$." Sky very hazy; the last measure of 4 marked very doubtful, but as they all agree fairly, the mean has been taken. The result differs from that in Dun Echt Circular, No. 10, and *Ast. Nach.* vol. 98, p. 325, by $-0^s{\cdot}19$ and $+3''{\cdot}0$. The result here given is the right one derived from the mean of the computed $\Delta\alpha$ and $\Delta\delta$. That previously given was obtained by taking the mean of the position angles and distances, a method which may lead to considerable errors when the described angle is large.

Nov. 16, 17, and 18. The comet could not be found owing to strong moonlight.

Nov. 19. The comet was fairly well seen, although the Moon was several degrees above the horizon. The condensation in the middle was tolerably well defined and could easily be measured. The comet did not seem to be perfectly round, but rather a little elongated on the following side.

Nov. 20. Comet perfectly round, considerably brighter in the middle, but without a real nucleus.

Nov. 24. To-day the comet is apparently brighter and more extended than on Nov. 20. Although very diffused and ill-defined it could be easily measured when the sky was perfectly clear.

Nov. 25. Measured with power 122. With 229 the texture of the comet is uneven as compared with a nebula. It has no nucleus, although an area in the centre 20″ in diameter is brighter than the rest. Obviously it cannot bear high powers, for it is apparently brighter and larger in the finder (Apert. 3¾ in.) than in the main telescope. There is no appreciable deviation from the circular form.

Nov. 27. The comet, though decidedly somewhat brighter than on the 25th, remains round and hazy as before.

Dec. 1. No change since Nov. 29. In the 6·06 in. Simms refractor, with a large Kellner eye-piece, the comet appears very large and tolerably bright, but without a nucleus. The diameter is 10′·5, being equal to the distance between D. M. +51°757 and 760. The coma is so faint as only to be visible with very low powers, hence the remarkable result that its *apparent* size decreases with increased magnifying power. Thus, in the 15·06 in. refractor it is seen as a small faintish comet; in the finder it is already considerably larger and brighter, while in the 6·06 in. Simms it is quite a striking, though not very bright object.

Dec. 8. Clouds troublesome.

Dec. 9. Measures easy, the comet being still tolerably bright, considering the already strong moonlight.

1881, Jan. 4. The comet is exceedingly faint and very difficult to measure, as the smallest star involved renders it almost impossible to say where the centre is. The condensation in the middle is very insignificant.

Jan. 6. The comet is so exceedingly faint as only to be recognizable in a perfectly dark field; the measures are in consequence very difficult,

All the measures were made with power 122, except on Jan. 6, when power 229 was also used. Each result is based on at least four comparisons. Observers, Ralph Copeland and J. G. Lohse.

Dun Echt Observatory,
1881, *Jan.* 13.

Further Researches on the Annual Parallax of 61 *Cygni*. By Professor Robert S. Ball, LL.D., F.R.S., Royal Astronomer of Ireland.

In the *Dunsink Observations*, Part III., pp. 16–39 ("Observatory," No. 17, vol. II., p. 137), I have discussed a series of measures of the difference between the declination of 61 A *Cygni* and a star of the 9–10 magnitude north following. The discussion of these observations gave an annual Parallax of $+0''\cdot4654\pm0''\cdot0497$. Shortly after this series was completed I commenced a new series, in which I employed the following star of 61 *Cygni*, often known as 61 B *Cygni*, instead of the preceding star which had been used in the previous researches. The same comparison star was employed, and the methods of observation and reduction were precisely the same. It is the object of the present paper to describe these observations, and to set forth the results to which they have led.

The series now under discussion was commenced upon September 18, 1878, and was concluded on October 2, 1879.

On each night of observation it was sought to take four complete measures of the difference of declinations. Each measure was derived from four readings of each of the screws in the direct position and four in the reversed position. The final result adopted for each night was the mean of the four separate measures.

To illustrate the nature of these observations and the method employed in their reduction, I here give a series of measures on a night, November 16, 1878, which I have selected at random from the entire series :—

Micrometric Measurements of the Difference of Declination of 61 *B Cygni and Comparison Star on the Night of November* 16, 1878.

Screw I.	Screw II.	Screw I.	Screw II.	Screw I.	Screw II.	Screw I.	Screw II.
63·698	40·627	55·783	32·139	64·318	40·047	55·599	32·446
63·753	40·598	55·857	32·134	64·163	40·177	55·562	32·506
63·791	40·537	55·902	32·063	64·175	40·237	55·501	32·549
63·823	40·537	55·921	32·074	64 097	40·257	55·483	32·633
55·639	32·356	64·217	40·184	55·906	32·143	63·557	40·863
55·666	32·353	64·215	40·142	55·697	32·293	63·293	41·103
55·732	32·268	64·202	40·113	55·716	32·303	63·267	41·088
55·753	32·262	64·213	40·095	55·708	32·348	63·154	41·228

Each of these eight columns has first to be reduced to seconds. This is accomplished by adding together the four larger numbers in each column, and subtracting from the result the sum of the four smaller numbers : *e.g.*, for the first column this result is 32·275. This has to be multiplied by one-eighth of the value of the micrometer Screw I., at the temperature of

the instrument (39°·9). The result thus found is 36″·292. In a similar manner the movement of Screw II. in the second column gives 37″·224. Thus the first set of measures on the night in question gives for the apparent total distance the result 73″·516. This must, however, receive a correction of +0″·026 on account of refraction, and of −0″·031 on account of reduction for Aberration, Precession, and Nutation,* so that the final result is 73″·511. In a similar manner we have for each of the other sets of observations on the night in question 73″·705, 73″·541, 73″·453. The mean of the whole four is 73″·552, which is accordingly taken as the final result of the observations for this one night.

In some cases the number of observations made was less than four, so it has been found convenient when estimating the weight attached to each night's work to employ simply the number which denotes the number of complete measures which have been obtained. Thus, for the night just considered, the weight is taken as 4.

In the following table is given the results of the observations on the several nights which are included in the present series:—

Mean Results of the Observations of the Difference in Declination between 61 B Cygni and Comparison Star reduced for refraction and reduction.

Date.	Diff.	Date.	Diff.	Date.	Diff.
1878.	″	1878.	″	1879.	″
Sept. 18	73·657	Nov. 17	73·609	Apr. 16	72·348
20	73·633	21	73·507	30	72·117
23	73·579	25	73·615	May 4	72·229
25	73·613	28	73·437	8	71·907
Oct. 23	73·520	Dec. 1	73·554	15	71·602
24	73·698	5	73·436	22	71·640
25	73·528	8	73·442	June 10	71·530
28	73·739	30	73·268	15	71·345
29	73·537	1879.		28	71·169
31	73·707	Jan. 8	73·400	July 26	70 853
Nov. 1	73·802	15	73·370	Sept. 17	70·424
8	73·633	18	73·325	Oct. 2	70·816
16	73·552	Apr. 10	72·512		

A glance at these results exhibits in a conspicuous manner the large proper motion of 61 B *Cygni* relatively to the star which has been chosen for comparison. To clear the observations from the grosser part of the effects of proper motion, it

* The methods of correcting such observations for refraction and reduction are similar to those already described in *Dunsink Observations*, Part III. pp. 19–22.

became necessary to adopt an approximate value of the relative annual proper motion in declination. The former series of observations had conclusively established the fact that the comparison star is not affected by any considerable proper motion. I therefore felt warranted in assuming Argelander's value of the absolutely proper motion of 61 B *Cyg.* in declination as approximately the relative proper motions. The value in question is $+3'\cdot016$ (*Positiones Mediæ*, p. 27).

The mean epoch of the entire series of observations is 1879·069. The mean value of the observed differences of declination is $72''\cdot85549$. The assumed epoch is 1879·0, whence applying a correction of $+\cdot20865$ for proper motion, we have as the adopted mean difference of declinations at the epoch $73''\cdot06414$.

The equations of condition are now formed in the usual manner.

The true mean distance at the epoch is assumed to be $73''\cdot06414-x$; the true value of the proper motion in declination is assumed to be $3''\cdot016+x'$; the correction to be applied to an observed difference of declinations for the parallax ϖ is $-[9\cdot92229]\cos(\odot-121°\ 39')\mathrm{R}\varpi$; the correction to be applied for a possible difference κ between the coefficients of aberration of the two stars is $-[9\cdot92229]\sin(\odot-121°39')\kappa$.

61 B Cygni.

Equations of Condition.				Weight.	Residual.
$-x+\cdot2868x'+\cdot4907\varpi+\cdot6785\kappa-\cdot272=0$				2	$-\cdot042$
$+\cdot2814$	$+\cdot4670$	$+\cdot6947$	$-\cdot280=0$	2	$-\cdot060$
$+\cdot2732$	$+\cdot4301$	$+\cdot7177$	$-\cdot309=0$	2	$-\cdot103$
$+\cdot2675$	$+\cdot4041$	$+\cdot7325$	$-\cdot258=0$	2	$-\cdot062$
$+\cdot1910$	$+\cdot0177$	$+\cdot8360$	$-\cdot120=0$	1	$-\cdot092$
$+\cdot1882$	$+\cdot0026$	$+\cdot8362$	$+\cdot066=0$	4	$+\cdot087$
$+\cdot1855$	$-\cdot0111$	$+\cdot8361$	$-\cdot096=0$	2	$-\cdot080$
$+\cdot1773$	$-\cdot0545$	$+\cdot8344$	$+\cdot140=0$	2	$+\cdot135$
$+\cdot1745$	$-\cdot0697$	$+\cdot8332$	$-\cdot053=0$	2	$-\cdot064$
$+\cdot1692$	$-\cdot0973$	$+\cdot8304$	$+\cdot133=0$	4	$+\cdot110$
$+\cdot1665$	$-\cdot1116$	$+\cdot8286$	$+\cdot236=0$	4	$+\cdot206$
$+\cdot1473$	$-\cdot2110$	$+\cdot8085$	$+\cdot125=0$	2	$+\cdot048$
$+\cdot1255$	$-\cdot3198$	$+\cdot7710$	$+\cdot109=0$	4	$-\cdot020$
$+\cdot1227$	$-\cdot3332$	$+\cdot7652$	$+\cdot175=0$	4	$+\cdot040$
$+\cdot1122$	$-\cdot3838$	$+\cdot7403$	$+\cdot105=0$	2	$-\cdot053$
$+\cdot1009$	$-\cdot4355$	$+\cdot7101$	$+\cdot247=0$	4	$+\cdot063$
$+\cdot0928$	$-\cdot4718$	$+\cdot6858$	$+\cdot093=0$	4	$-\cdot110$
$+\cdot0843$	$-\cdot5074$	$+\cdot6589$	$+\cdot236=0$	4	$+\cdot016$
$+\cdot0735$	$-\cdot5511$	$+\cdot6214$	$+\cdot150=0$	4	$-\cdot092$

Equations of Condition.				Weight.	Residual.
+·0652	−·5832	+·5902	+·181 = 0	4	−·079
+·0054	−·7587	+·3222	+·188 = 0	4	−·167
−·0200	−·7996	+·1947	+·396 = 0	2	+·015
−·0391	−·8175	+·0922	+·424 = 0	4	+·029
−·0473	−·8214	+·0481	+·404 = 0	2	+·005
−·2730	−·1566	−·8215	+·271 = 0	1	+·140
−·2920	−·0717	−·8331	+·165 = 0	1	+·074
−·3274	+·1275	−·8265	+·040 = 0	2	+·047
−·3383	+·1831	−·8162	+·185 = 0	4	+·219
−·3491	+·2382	−·8022	−·104 = 0	1	−·043
−·3685	+·3332	−·7686	−·351 = 0	2	−·241
−·3876	+·4226	−·7246	−·255 = 0	2	−·098
−·4394	+·6307	−·5599	−·209 = 0	2	+·082
−·4530	+·6763	−·5061	−·353 = 0	1	−·056
−·4887	+·7717	−·3507	−·421 = 0	2	−·065
−·5657	+·8485	+·0297	−·505 = 0	4	−·079
−·7100	+·5067	+·6669	−·499 = 0	2	−·175
−·7516	+·3162	+·7741	+·019 = 0	2	+·266

From these I obtain the following normal equations:—

$$
\begin{aligned}
+97{\cdot}000\,x + 3{\cdot}4413\,x' + 10{\cdot}009\,\varpi - 36{\cdot}235\,\kappa - 3{\cdot}9766 &= 0\\
+\ 3{\cdot}4413 + 7{\cdot}5700 - 5{\cdot}9279 + 9{\cdot}0188 + 2{\cdot}7095 &= 0\\
+10{\cdot}009 - 5{\cdot}9279 + 22{\cdot}683 - 11{\cdot}831 - 10{\cdot}504 &= 0\\
-36{\cdot}235 + 9{\cdot}0188 - 11\,831 + 44{\cdot}890 + 3{\cdot}9185 &= 0
\end{aligned}
$$

Solving these equations I obtain

$$
\begin{aligned}
x &= +0''{\cdot}0227 \pm 0''{\cdot}0194\\
x' &= -0{\cdot}0878 \pm 0{\cdot}0705\\
\varpi &= +0{\cdot}4676 \pm 0{\cdot}0321\\
\kappa &= +0{\cdot}0719 \pm 0{\cdot}0314
\end{aligned}
$$

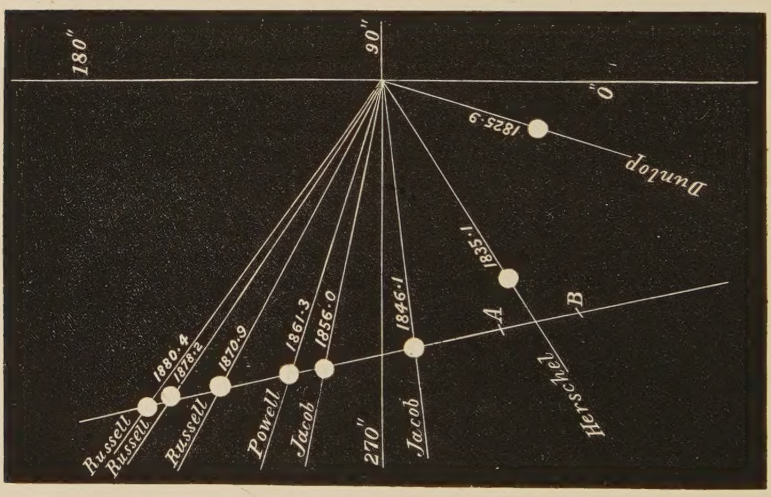
These values when substituted in the equations give the residuals in the column following the equations of condition. The sum of the squares of the absolute terms is 6·172, while the sum of the squares of the residuals is but 1·223, the weights being of course taken into account in each case.

The probable error of one observation is $\pm 0''{\cdot}1298$, whence the probable error of the mean of four observations, which form a complete series for one night, is $\pm 0''{\cdot}0649$.

The agreement of the present parallax of 0″·4676 with that of 0″·4654, found by the former series, is no doubt satisfactory. The accordance is indeed so close, that it is clearly to some extent due to the chapter of accidents. It would, however, seem that these investigations render it probable that the annual parallax of 61 *Cygni* is nearer the half-second found by Struve than the third of a second found by Bessel.

Southern Double Stars. By H. C. Russell, Esq., Director of the Sydney Observatory.

I have recently found that all my own observations of *p Eridani*, the interesting Southern double star, are better represented by a straight line than by the orbit which it has been supposed the companion star was moving in; and upon collecting other observations I found that as far back as 1846 the motion seems to have been in a straight line. Further, the motion in this line seems



by the observations to have been almost exactly at the same rate from 1846 to 1856 as from 1870 to 1880. If this rate of motion is carried back to Herschel's date we get a position marked A in the accompanying diagram, which shows the position *p Eridani* would have then occupied; if its motion is in a straight line this only involves the supposition of a small error in angle, and half a

second in distance, in Herschel's observations, quantities not impossible considering the moderate optical power at his command. With regard to Dunlop's position it appears, on reference to his notes, that he did not measure the distance; in fact, he only *estimated* it, and his telescope was so inferior that he may easily have made an error of half the amount. This seems the more probable from the fact that he made the angle of position 16° 54′, which does not accord with the subsequent measures by other observers. It does not seem, therefore, unreasonable to suppose that at his date *p Eridani* was at B in the diagram. Altogether the evidence is very much in favour of the supposition that p^1 is simply p^2 by reason of its proper motion.

If, however, Dunlop's observations must have full weight attached to it, then errors must exist in the work of other observers before the positions will fit into any orbit; and that of Jacob, *Monthly Notices*, vol. x. page 170, is much too small. In my own observations, made with a telescope of $11\frac{1}{2}$ inches and power of 800, I do not think there can be any error sufficient to account for orbital motion.

An examination of meridian observations shows that in 1840 *p Eridani* was observed as one star at Madras, and the position given, R.A. $1^h\,33^m\,43^s{\cdot}91$ and N.P.D. 147° 0′ 29″·45, probably refers to the mean of the two stars. This N.P.D. brought up to 1875 agrees very well with that of p^2 in the Cape observations, but p^1 has increased in R.A., and in N.P.D. is 3″·56 greater; this agrees with the equatoreal measures, and goes to prove that p^1 is moving.

I can detect little, if any, difference in magnitudes; if anything, p^1 is the smaller. *Dunlop*, *Herschel*, *Jacob*, and *Powell* made the stars equal, and the Cape Catalogue is the first place I have seen a difference in magnitude noted.

The following recent measures of other southern doubles may be of interest.

Date of Observations.	Name of Star.	R.A.	Angle of Position.	Distance.	Power used.
		h m	° ′	″	
1880·408	κ Toucani	1 11	0 24	5·64	300
1880·446	γ Centauri	12 34	181 16	1·39	800
1880·446	α Centauri	14 31	184 59	5·52	800
1880·446	π Lupi	14 56	189 18	0·90	800
1880·457	H 5014	17 58	79 18	0·80	800
1880·457	γ Coronæ Aust.	18 58	53 8	1·15	800

p Eridani.

Observer.	Epoch.	Mean Epoch.	Position.	Mean Position.	Distance.	Mean Dist.
Dunlop		1825·90		343·10		2·500
Herschel *	1834·85	1835·14	302·33	301·68	3·65	3·68
	1835·		302·30		3·65	
	1835·58		302·42		3·75	
Jacob	1845·87	1846·11	276·0	276·25	4·16	4·21
	1845·87		276·2		4·16	
	1845·88		275·8		—	
	1846·83		277·0		4·32	
Powell		1853·95		263·10		3·95
Jacob	1856·08	1856·03	262·11	259·68	4·92	4·62
	1856·09		260·06		4·63	
	1856·10		261·47		4·62	
	1857·90		257·18		4·38	
	1857·98		257·60		4·56	
	1858·02		258·93		4·55	
Powell		1861·34		253·37		4·86
Russell, Mean of 10 Obsvns.		1870·92		242·60		5·46
,, Power 800, 10 Obsvns.	1878·16	1878·19	236·34	237·00	6·029	6·138
	1878·22		237·26		6·147	
,, Power 800, 10 Obsvns.		1880·45		234·41		6·295

* 180° has been added to angle.

Sydney Observatory,
1880, *June* 17.